

Report to Paul Hertz on the Results of the Community Survey on a Possible Delay in the 2020 Decadal Survey on Astronomy and Astrophysics

Paul Scowen, John O'Meara, John Conklin, Victoria Meadows, and Susan Neff on behalf of the Executive Committees of the COPAG, PhysPAG and ExoPAG

Preamble

Due to the delay in JWST and the proposed termination of WFIRST, Thomas Zurbuchen, Associate Administrator Science Mission Directorate and Paul Hertz, Astrophysics Division Director were concerned that the next decadal committee may not be able to effectively prioritize missions in the next decade due to uncertainties in the status of JWST and WFIRST. They have suggested that one way to resolve that concern would be to delay the next Astrophysics Decadal Survey by about two years. However NASA decided to seek thoughtful input from the community on whether there is another option –

- **Can we have an effective decadal survey even in the context of the uncertainties in the status of JWST and WFIRST?**
- **What intentional steps can we take in conducting the decadal survey to ensure that the decadal committee can effectively assess the highest science priorities and recommend a balanced program of activities and missions for the coming decade?**

The Survey

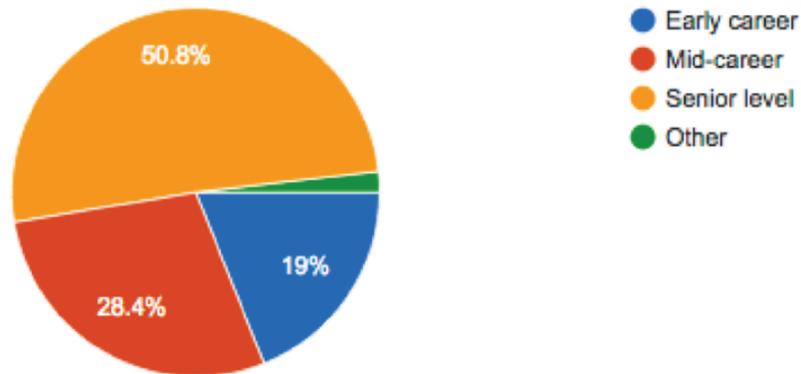
In response to this query, the Cosmic Origins Program Analysis Group (COPAG) Executive Committee (EC), in partnership with the Physics of the Universe PAG (PhysPAG) and the Exoplanet PAG (ExoPAG), assembled a simple survey to allow the community at large to give input on the questions raised above – this survey was released on May 3, 2018, although the PhysPAG community did not receive this notice until May 8. The survey is included as Appendix B and was closed for input on May 13, 2018.

The short period of time for the survey was necessary to allow a report to be filed with Paul Hertz before the anticipated response of the Independent Review Board that is considering the impact of the JWST delay. Because of this fact it should be recognized that the survey likely captures the strongest opinions and, for example, those respondents that had time at the end of the academic semester to fill out such a survey. In addition it should be noted that the accelerated schedule required the survey to be administered without a supporting town hall type activity where the pros and cons could be discussed, and that there was not an opportunity to do a test for biases.

The Response

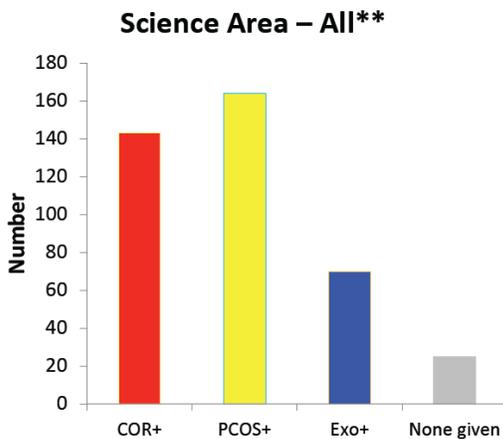
Initial demographic and multiple choice responses from the community included the following:

- 329 individual responses
- Professional status (optional, 329 responses):
 - Early career: 19%
 - Mid career: 28%
 - Senior career: 51%
 - Other/None: 2%

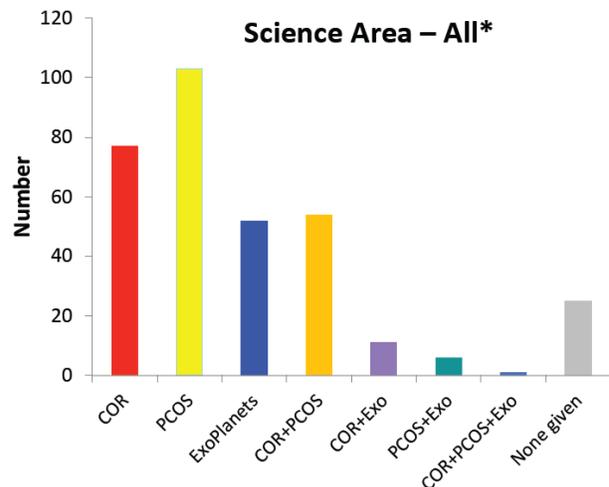


And broken out by interest area between the three PAGs represented:

- Preferred science area(s) (could choose more than 1, optional, 385 responses if you count, e.g., COR + PCOS as 2 responses):
 - Cosmic Origins: 143 (47%)
 - Physics of the Cosmos: 164 (54%)
 - Exoplanets: 78 (26%)

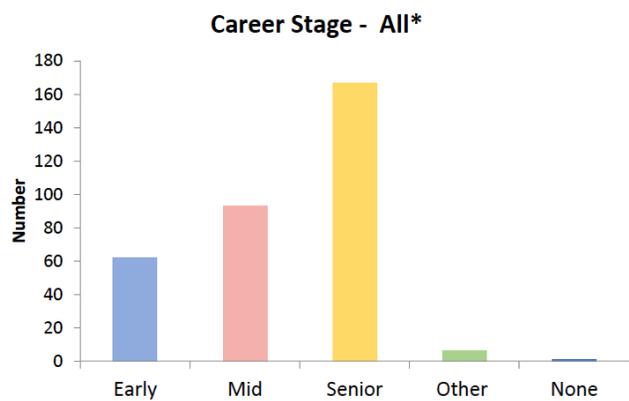


** In this plot, some people are counted 2 or 3 times, e.g., COR+ = COR, COR+PCOS, COR+Exo, and COR+PCOS+Exo: 385 total responses.

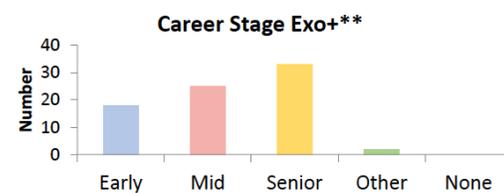
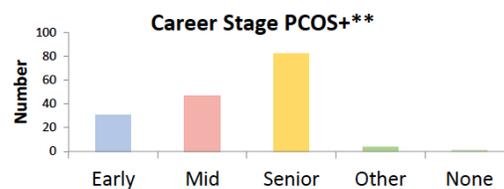
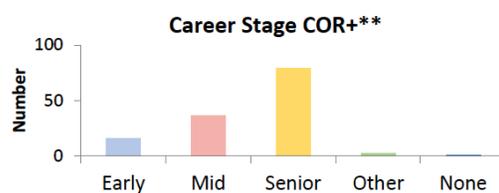


* In this plot, each person is counted once: 329 responses

Concerning the career stage of respondents:



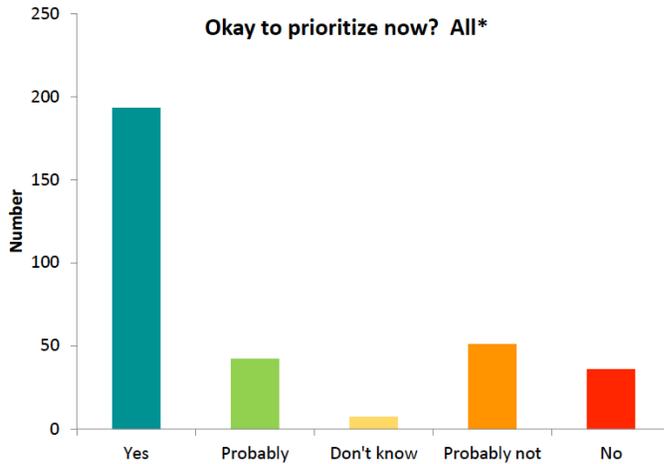
* In this plot, each person is counted once: 329 responses



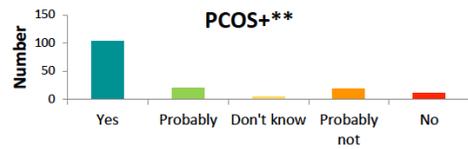
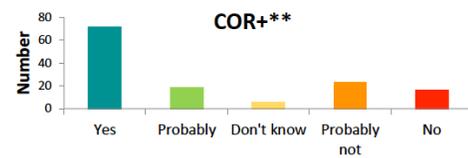
**In these plots, some people are counted 2 or 3 times, e.g, COR+ = COR, COR+PCOS, COR+Exo, and COR+PCOS+Exo. 385 total responses

On the primary question: Do you think it will be possible to prioritize astrophysics space science goals for the next decade before JWST's operational status is known?

- (329 responses, only one answer allowed)
 - Yes: 58.7%
 - Probably: 12.8%
 - Don't know: 2.1%
 - Probably not: 15.5%
 - No: 10.9%
- (Yes + Probably: 71.5%)
- (No + Probably not: 26.4%)

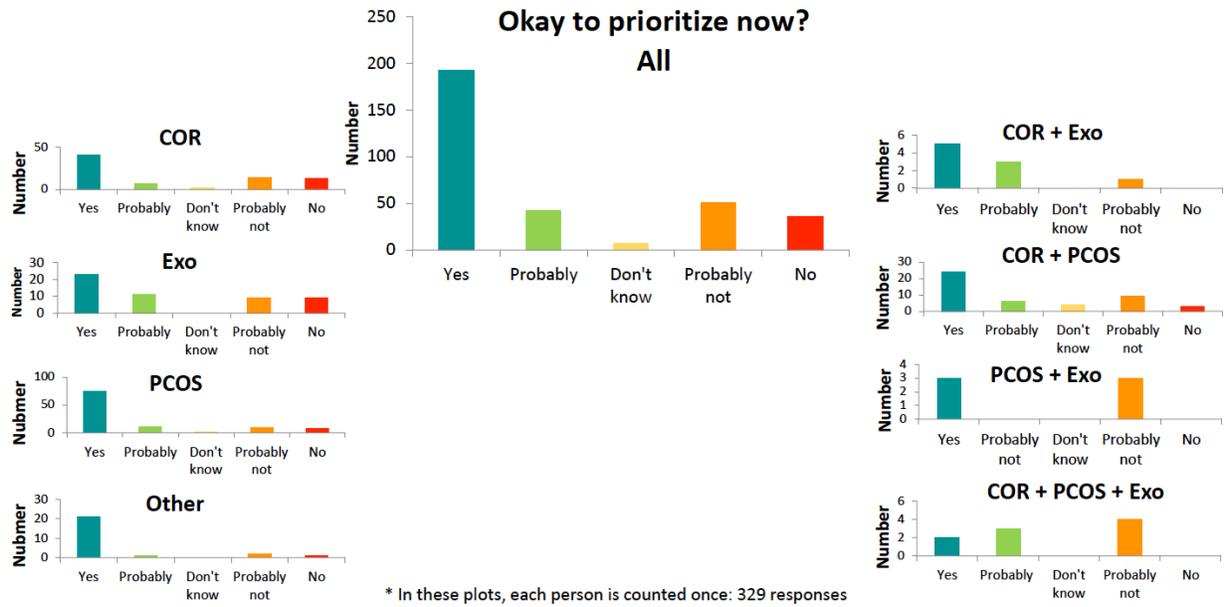


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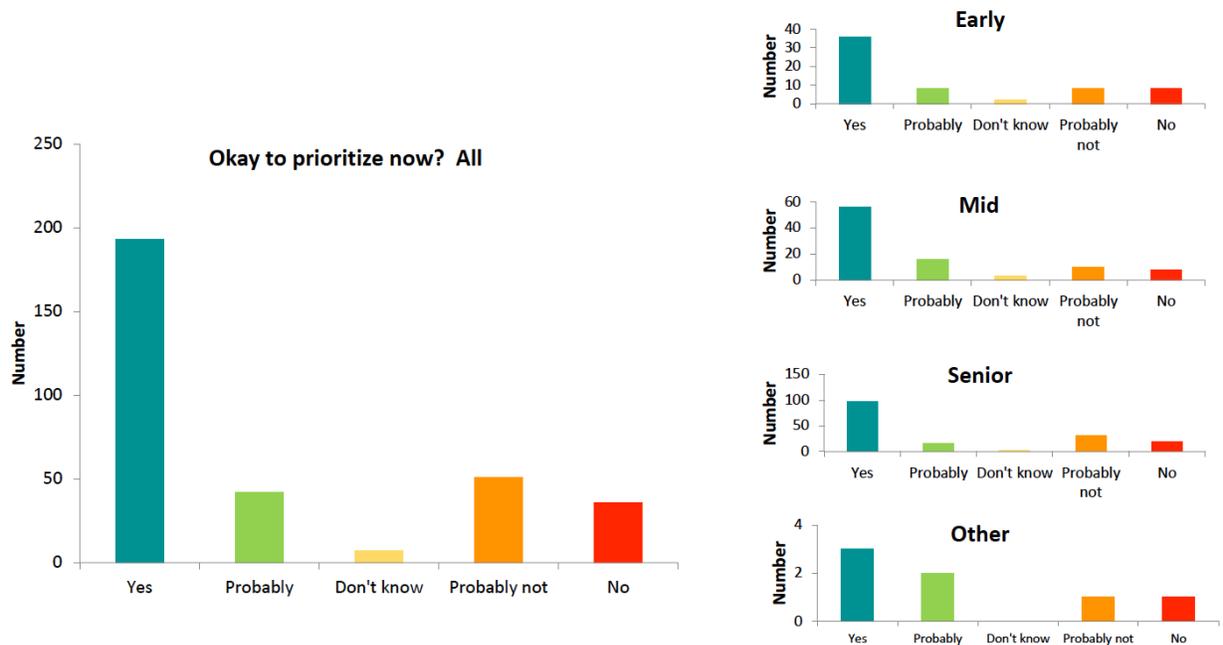


**In these plots, some people are counted 2 or 3 times, e.g., COR+ = COR, COR+PCOS, COR+Exo, and COR+PCOS+Exo. 385 total responses

And broken down even further:



Breaking out the response by career stage gave these distributions:



Question 1: Do you think it will be possible to prioritize astrophysics space science goals for the next decade before JWST's operational status is known? Why or why not?

Analysis of the resulting comments yielded the following observations. Exemplar comments are used to characterize the theme in each case.

- “There’s more to science than JWST” - Lots of people focused on JWST’s wavelengths and science portfolio and argued that as a result the Decadal should go on as scheduled for the reason that JWST only represents a fraction of what the community does.
- “Either JWST works or it doesn’t” - This was a frequent argument made to keep the Decadal on its original schedule. The argument being that few high-impact science results from JWST will have been forthcoming even if the 2-3 year delay being proposed for the Decadal is enacted.
- "JWST is too important, and we must delay" - A minority opinion, but a frequent one made by that minority.
- “JWST and WFIRST are going to eat astrophysics” – Such comments were usually coupled with a request to continue the Decadal as scheduled.
- “The science is changing rapidly” – Or, put another way, don’t delay the Decadal because some science (e.g. exoplanet science, transient science) needs some prioritization now.
- “We shouldn’t be held hostage to any one mission” - self explanatory. The idea here is that while JWST will impact certain facets of astronomy, it will not impact the pivotal science questions for a large part of astronomy and astrophysics. Put another way, the success or failure of one project or mission should not be allowed to divert the community from the decadal ritual of taking stock.
- "Delaying sets a dangerous precedent" - self explanatory.

Question 2: What advantages or dangers do you see to doing the Decadal Survey on the current schedule, vs. delaying the DS by approximately 2 years? (Examples might include: loss of momentum for 2010 DS priorities, ID of new science questions or technology challenges that require immediate attention/prioritization to avoid losing US leadership, better insight into new capabilities such as launch vehicle capability, impact on already planned activities, ...)

The range of opinions expressed are grouped together again:

- If delayed, it represents too long a period of time without guidance for the community from the National Academy – the community as a whole will lose momentum in their strategic development. In particular the ground-based astronomical community needs to prioritize where its shrinking budget should be invested – a delay would be catastrophic. Guidance is also needed in particular reference to the emerging field of Gravitational Wave astronomy, and the development of the 30m-class telescopes.
- If conducted on schedule, there is concern that the Survey will regard large or Flagship missions as being too hard to do on schedule and on budget. If this happens, and money is moved out of such development, it may prove impossible to get back.
- There are numerous grass roots efforts already underway to self-organize the community to deliver input to the Decadal next year, not to mention the large mission and probe-class mission studies being conducted by NASA itself. If the Decadal is delayed all these efforts will be for naught as the science and technology state-of-the-art will be stale or out of date by the time the Decadal convenes.
- If delayed, the perspective for the field with a proven successful JWST will be very different and may change priorities looking forward into the next decade. Similarly if JWST does not work or has a failure of some kind, that same landscape will be changed in a different way – and worst case, we might be left with nothing and no plan to move forward with. No one mission should dictate the direction of the entire field, or the programmatic problems of NASA.
- We need to be careful about what message we send to Congress about our strategic directions. If we appear to be incapable of setting our priorities because of uncertainty about the future then our ability to carry out a rational science and technology program may be put into question.
- If we delay setting our plans forward through the Decadal, the rest of the world will advance without us and we may lose our pre-eminent position in the global field of astronomy and astrophysics. Other countries are not going to wait for us.
- If delayed, what is the criteria for the “right moment” to do the Decadal? First ERS observations from JWST? First science observations? First papers to come from those observations? The exit criteria have not been defined and as such this looks like an indefinite delay.
- NASA appears to be getting out of sync with the Decadal cycle because of its failure to deliver missions on schedule, and that may be motivating this delay request. It might be a better thing for NASA to finish WFIRST on budget and schedule with some smaller missions and leave the next large missions to be considered to the 2030 Decadal.

The Outcome

We sought to answer the following two questions with this survey:

- Can we have an effective decadal survey even in the context of the uncertainties in the status of JWST and WFIRST?
- What intentional steps can we take in conducting the decadal survey to ensure that the decadal committee can effectively assess the highest science priorities and recommend a balanced program of activities and missions for the coming decade?

The outcome of the survey is that the majority of the community wants the Decadal survey to go on as scheduled even in the face of the above-mentioned uncertainties associated with JWST and WFIRST. There is a common opinion that delaying the Decadal would render it less effective and do real and lasting damage to other facets of the astronomical community. As such the community's answer to the first question is:

- Yes, the Decadal Survey can be conducted effectively on the original schedule even in the face of the uncertainties associated with JWST and WFIRST (Yes + probably: 71.5% vs. No + Probably not: 26.4%).

In the interests of completeness, the community's minority views on this question were as follows:

- If executed on schedule the Decadal Survey will not know the status of the previous two Decadal Survey's top recommendations. In addition there will be an atmosphere of skepticism about NASA's capabilities to complete large missions. Future science priorities will be significantly affected by JWST results and status. We will also have no idea of the quality of JWST data. JWST success will be needed to provide the underpinning for exoplanet atmosphere and biosignature research.

As to the second question, the point was made by many respondents that to achieve the balanced approach of outlining activities and missions for the coming decade, we must not allow the entire Decadal Survey process to be up-ended by delays in one mission. It was remarked that to consider such a delay undermines the very assumption that the program we are pursuing is in fact balanced. As to the uncertainty associated with JWST and what that might do to a future balanced program – many respondents simply stated that it is a risk we must carry into the next decade, but we must not let it delay and cause a loss of momentum in other aspects of astronomy. It was a common observation that to do so risks losing the leadership role that NASA has enjoyed to date. So, the community's answer to the second question is:

- We should proceed with the Decadal Survey as scheduled and carry the risks associated with JWST and WFIRST as possible successes or failures, but we should not allow those risks to stop the advance of the field – a field that is far larger than space-based OIR

astronomy. This will provide the balanced program the community wants, and it will identify the highest science priorities whether or not JWST is operational when the Decadal convenes.

As before, in the interests of completeness, the community's minority views included:

- While one mission should not affect the priorities of the community as a whole – the unprecedented cost of JWST should require an exception in this case. Technology development also needs more time to achieve maturity. Any delay should be kept to less than a year. The status of 30m-class ground based telescopes will be clearer in 2023 than in 2020. A delay would allow a clearer view of the budgetary “free energy” once JWST has launched and WFIRST's fate has been decided. Again, the status and performance of the just-launched JWST will be critical in defining what the next large mission should look like.

Appendix A:
Responses to PAG Survey
re. Decadal Survey and JWST

23 May 2018

	A	B	C	D
	Do you think it will be possible to prioritize astrophysics space science goals for the next decade before JWST's operational status is known?	Why or why not?		What advantages or dangers do you see to doing the Decadal Survey on the current schedule, vs. delaying the DS by approximately 2 years? (Examples might include: loss of momentum for 2010 DS priorities, ID of new science questions or technology challenges that require immediate attention/prioritization to avoid losing US leadership, better insight into new capabilities such as launch vehicle capability, impact on already planned activities, . . .)
1	Probably	Probably - this seems redundant		Too long a period of time with no guidance. Too many groups that have started preparing material but would have to shelve it for 3 years.
2	Probably not	If executed as planned, the decadal will not know the status of both the previous top recommendations over two decades. Moreover, they will be operating in a climate of extreme skepticism about NASA's ability to execute large missions on time/on budget, irrespective of the significant work the large mission concept teams are doing in advance of the survey.		The ground based community will benefit from having the decadal now. Of primary concern to me is a decadal that views large missions as too hard to do right now, and will punt to the next decade, thus severely endangering US leadership in space astrophysics, along with running the significant risk that Congress will move the money out of NASA APD, or out of NASA entirely. Bringing that \$ back a decade from now is hardly a guarantee. Delaying the decadal gives more time to mitigate technology risk, and better understand the heavy lift landscape as well. In the interim, NASA could commit to adding another astrophysics SMEX or MIDEX to the list, so as to keep the community in motion.
3	Probably	A significant question will be the balance between Flagships and Probes, while maintaining a strong Explorer line.		If there are further slips to the JWST schedule, we would lose an entire decade.
4	Don't know			
5	Don't know			
6	Probably not			
7	Probably	There are other missions such as XARM and, possibly, LUVOIR that can be considered.		loss of momentum for established priorities is a major concern.
8	Yes	JWST is important, but it is not the sum total of everything we do in space science. I think it is more important to keep the cadence and momentum of the decadal surveys, with a success-oriented vision whereby we assume that JWST performs up to spec, then to delay the process. If JWST fails, we (the astrophysics community) will have bigger problems than just the fact that aspects of the decadal survey may be obsolete.		As I argued above, we cannot leave the Decadal Survey process hostage to JWST. The loss of momentum would be severe, and would affect any number of other major efforts throughout our community.
9	No	JWST is the most advanced telescope ever built and will revolutionize entire themes of COPAG science, and likely discover new phenomena. Planning the Decadal survey after JWST science results come out would give the community much more insight in what to prioritize over the next ten years.		I see a major danger in moving forward with the Decadal Survey before JWST in that we may lock in science priorities related to COPAG themes such as star formation and galaxy evolution that are largely addressed by JWST. Understanding what to do next requires us to first see what JWST reveals.
10	Probably not	No 1-25 micron science prioritization can take place without knowing JWST performance and first 6-12 months of science results.		Advantage of current schedule: It's a bad precedent to have the issues of one single project affect the decadal schedules. But JWST is so unprecedented in cost that it is worth doing in this case.
11	Probably not	JWST is so critical that it is essential to know its outcome before prioritizing the next steps.		I think we will be better positioned to advocate future plans once we have a success with JWST.
12	Probably	The decadal could proceed under the assumption that JWST will launch successfully in ~2020. But if JWST were to fail we would need to completely re-evaluate.		We need to evaluate ground and space based priorities in light of the new, gravitational wave era.
13	Yes			
14	Probably not	The field of exoplanets advances fast, so a couple years launch delay can change the main science focuses.		Delaying it will help identify more up to date science questions.
15	Yes	The 2010 decadal prioritized future missions in the face of significant uncertainty associated with JWST's timescale and budget. That seems harder to deal with in decadal planning compared with uncertainty due to JWST's performance.		Loss of momentum for the non-JWST aspects of astrophysics, including ground-based astrophysics planning, that provide important scientific balance. Delay of promoting emerging themes such as transient astronomy and probe missions.
16	Yes	Astro 2020 can recommend mission priority decision rules based on the success or failure of JWST. My understanding is that the NAS and NSF do not want to delay Astro 2020 for their own obvious reasons of wanting to keep this process moving on the long-planned time schedule.		See above.
17	Don't know	Although JWST capabilities may influence the outcome of the decadal – the answer to this question is dependent on the resources (time/budget) required and/or allocated to achieve operational status. As a member of the community – it is not readily apparent how well these variables are presently known.		Slippage will increase the end cost of non-JWST science priorities in the pipeline – arguably – in a climate of constrained resources this would be undesirable.
18				

	A	B	C	D
19	Yes	The finish of the decadal is scheduled for Dec 2020. Most of the work can be done with the anticipation JWST will launch with expected capability within 2020. If something unexpected happens (diminished capability, delayed launch), then the final report can be delayed/amended rather than not starting until after the launch. The delay will be less significant to the 2020 priorities.		There is a significant loss for space priorities and missions from a two year delay. The current Astrophysics Mission budgets have already experienced a significant delay from previous JWST funding problems, it seems as if this would add one more significant hit to its legacy before it even launches.
20	No	JWST is very likely have major new discoveries and provide huge leap in our understanding of important astrophysics problems. These have to be taken into account when deciding the next astrophysics mission and even MIDEK level programs.		All of the above dangers are understandable and are important to take into account. But, at the same time future mission concepts (especially the flagship missions) rely on a successful launch and early science results of JWST.
21	Probably not	If it were any other mission, I would say that we should continue with the normal decadal review process. But it is *critical* to know whether JWST is launched and working properly before prioritizing additional missions. JWST is just too big, both from a science point of view and a funding point of view.		
22	No	So much future planning and strategizing depends upon JWST working. Not just from a science standpoint, but from technical details such as the deployable sunshield working (as one single example).		CCAT has already stalled out, and that was the top priority for mid-scale ground-based projects. LSST and ALMA are chugging along. I'd just wait until we see first light with JWST before making recommendations for NASA for the next decade.
23	No			A two-year time frame is likely too short for existing key science priorities to change dramatically or fundamental and potentially transformative new science questions/issues to emerge. Knowing whether or not JWST will be able to carry out, partially or in full, its mission, on the other hand, seems to be a critical piece of information to have before a global assessment of the U.S. astronomical community current standing, priorities and future directions are reviewed and evaluated.
24	No	priorities for new missions will depend on the schedule for JWST		danger of current schedule could mean lower priority put on future NASA missions (2030s+) if JWST is not even certain and potential loss of flagship program for NASA. delay will allow the most up-to-date science to drive the next big mission.
25	Probably not	JWST is both a major investment and capability. Like it or not, how JWST fares will impact the future options for much of our community.		The Decadal Surveys need to exercise extreme care to remain connected to reality in terms of budgets, scientific opportunities and community capabilities. A DS that is based on a weak foundation that would result from mis-judging the role of JWST will be starting in the wrong direction. It then may be extremely difficult to recover and produce a credible plan.
26	Probably not	Many of the goals of future missions will depend on the success of JWST		Delaying the survey will enable better insight into what missions will be required to achieve long-term community goals
27	Probably	Since JWST budget is separated from the rest of NASA Astrophysics, I do think it is possible to prioritize without thinking about additional budget for JWST in case of a delay, which we would have to fight for regardless of prioritization of other missions. Ensuring JWST's launch is, in practice, the very top priority already.		a) It may push the next decadal survey further... b) loss of momentum on the current work on this decadal survey. c) A delay in prioritization of missions/projects is likely to translate into a delay in getting them done.
28	Yes	Even if it fails, replacing JWST appears to be out of the question, therefore the priorities for the next decade will not be tightly coupled to the success or failure of JWST. If successful, it will take more than a year post-launch for the scientific impact of JWST (in terms of raising new questions and need for followup observations) to become apparent. The community cannot afford to delay the decadal survey by half a decade.		IMHO, the decadal survey can and should move ahead independent of JWST. It would be a mistake to make the Decadal survey hostage to the status of JWST.
29	Probably	Presumably any decadal in the < 2022 time frame will be too early to have knowledge of the biggest science surprises from JWST, whether the decadal is delayed or not. As a result I am skeptical whether the information to steer funding priorities beyond what we know today will be available. A delay beyond that time frame more advantageously leverages JWST, but leaves a vacuum elsewhere that is problematic.		My observation is that NASA astro's funding priorities for the next decade are more or less already laid out (WFIRST, Probe-class mission, Explorers). Would a decadal survey released in 2023 that recommends a completely new direction based on preliminary results from JWST actually move the ship? Perhaps, but I think it's more likely that it would just queue up the funding priorities for 2030. Perhaps it's better to do the decadal as planned and then a fairly comprehensive half-decadal report, and allow the latter to make recommendations for missions based on JWST, which matches the funding availability better as well.
30	Probably not	1. The amount of allowable mission risk will be perceived quite differently depending on the success of the JWST mission. 2. Future scientific priorities will depend significantly the availability of the JWST science		see answer to "why or why not"
31	Yes	The goals of the future do not necessarily rely on the existence and results of JWST. JWST is very specific and reflects the needs of a decade ago. These have changed significantly since then.		It will significantly impact current plans, will delay progress of the next missions.

	A	B	C	D
32	Yes	The Decadal Survey can make two recommended plans. 1. JWST works. 2. It doesn't.		It is important to do the decadal survey every decade. It would be very useful to have the recommendations for the survey near the beginning of the JWST mission. If the JWST launch slips again, and then the mission fails, we are in the position of being well into the decade with no plan. I have confidence the decadal committee can assess the two options based on a binary choice for JWST - works/fails. There is also more in the Decadal survey than NASA astrophysics. The astrophysics community is primed and ready to do the next decadal. We should stop waiting for JWST before determining our priorities.
33	No	The scientific goals of the 2000 Decadal Survey, embodied in JWST, would remain unfulfilled. How to achieve them would need to be reconsidered. Any follow-on mission designed to build upon them would have no basis.		I see only disadvantages to doing the survey on its current schedule, as explained above. Planning for a new decade without the foundational knowledge it is supposed to build upon is simply too optimistic.
34	Yes	There have always been constraints and pressures at the times of past Decadal surveys. JWST is a big shot, but I don't think we should see it as an excuse to postpone the upcoming survey.		I don't think that having a prioritized wishlist hurts at all. I am more concerned that any potential further delay of JWST could put us in a 10-year limbo if we decide to postpone the Decadal survey even once.
35	Yes	The decadal review should proceed assuming that JWST will be fully operational and can be taken advantage of. A sidebar on possible effects on future missions if it is not can be included. If it turns out that there are problems then special planning will occur depending on the nature of the problems. The community can handle that. See below about the dangers of not holding to schedule for the Decadal Survey,		We should not allow special circumstances to alter the pace and schedule of the decadal surveys. There will always be special circumstances that might warrant a change of schedule but once we set a precedent of waiting for the right time the value of the survey will be irreparably damaged. The astronomical decadal survey is one of the most powerful tools we have in convincing congress to fund our programs. If we appear to be incapable of setting our priorities because of uncertainty in the future that will be noted by congress and our ability to carry out a rational program will be questioned. Keep the survey on schedule.
36	No	Too many unknowns until JWST is launched and operational; prioritizing the next decade requires at least some solid information. Knowledge whether JWST is working at the expected level is one of such solid information.		I perceive many advantages, which, in my opinion, overshadow any of the disadvantages.
37	Probably not	A successful launch and initial operation of JWST is an essential piece of information in this process. If the mission fails, this could change the landscape.		The advantage of a delay would be that there will be more time for critical detector and readout technologies to be ready, especially for cryogenic detectors. One disadvantage is that other international agencies also take into account the US Decadal Survey in their decision making, and this would affect international and projects as well.
38	Probably	The cost overruns on JWST are very significant which can cause a whole class of missions to be excused from the evaluation e.g. a 6 month JWST delay is equivalent to a SMEX class mission. That said, the agencies do not approve everything that the committee recommends and it is better to know what the committee thinks the priorities are. For example, if they still think after these JWST and WFIRST tribulations that a flagship mission should be done, then we as a community can prepare ourselves for 1-2 decades without (m)any other small projects.		Biggest danger is we won't know what our priorities are for two more years - especially a big issue if we are going to join forces with one of the other major partners on a mission (e.g. SPICA, LISA, WFIRST). We can't expect the rest of the world to be waiting for our recommendations. I think we may as well save people's time and cancel the decadal then and keep working with the priorities from the last decadal given that the flagship mission from back then hasn't even gotten off the ground.
39	Probably not	Uncertainty in NASA funding levels for new missions.		Advantage: Clearer focus on new mission budgets in a post-launch JWST env.
40	Yes			There is little to be gained by delaying two years. Astronomy is very different today from 2010 and we should take the opportunity to adapt and plan sooner rather than later. JWST is important but astronomy does not fail on its delay. TESS is heading to orbit, GAIA is operational. Ground-based telescopes continue to discover. There's plenty to work with and culling and prioritizing the discovery space can be done effectively even hedging on JWST.
41	Probably	The timing may be off, but the prioritization is unlikely to change as long as we assume JWST will be successful.		I see a danger in delay by not allowing better ground-space coordination and advice to NSF on a schedule that makes sense for the ground. This is not just a NASA decadal survey.
42	Yes	JWST's operational capabilities may not be known fully for several years after launch. Nothing is certain with space missions. Is NASA committed to fix JWST if e.g. mirror does not deploy properly? Commission a half-decade survey which will hopefully be after JWST's operational status has been fully analyzed.		Advantage is the implicit statement that JWST is not all of NASA astrophysics. Danger is that Decadal Committee will focus on questions revolving around JWST rather than do its job.
43	Yes	JWST might never launch or might fail after launch and it seems a shame to be delaying people's science to wait for it.		

	A	B	C	D
44	Probably	JWST's capabilities and many of its discovery milestones are now not only well known, but—given the effort put into the ERS and Cycle 1 proposal programs—well understood by the community. Hence, in terms of programmatic prioritization or coordination, there is little qualitative advantage to delaying the Survey. The risk of catastrophe remains, but this has always been the case, and I doubt any roadmap that could emerge so soon after such an event would have little long-term practical utility. Lastly, the field has immediate/acute concerns that should be addressed as quickly as possible. These include the status of the two US-led GSMT facilities, and WFIRST, both of which face real financial and other pressures whose discussion should not be delayed. Basically, JWST is not the be-all/end-all of the field, which has problems that should be addressed now irrespective of that facilities status.		See prev. answer.
45	Yes	Most of the multi-wavelength/multi-messenger/wide-field space-based projects we need to prioritize do not necessarily depend on JWST. We will lose enthusiasm and momentum for numerous projects, both ground and space based, if we delay the Decadal Survey.		
46	No	As we know there is high risk in this inherently complex observatory. We also don't know the full extent of the hardware problems causing the delay. It is likely to be more severe than known in public and there is a good chance of more serious hardware problems popping up. There is also the possibility of problems during and after launch. If JWST is unsuccessful then this will lead to wasted time and money if planning is based on a successful deployment of JWST.		
47	Don't know	This telescope is a very significant a change in astronomy capabilities, and has much potential to change NASA budgets compared to whatever assumptions are made. Of course, its first results will also make dramatic changes to many fields of inquiry, so it's not clear how long we'd have to wait to include its full impact. Nevertheless, the connection between the decadal survey and the implemented missions/projects seems weak and short-lived (except to the extent that it kills certain nasa candidate missions) so perhaps it is better to maintain schedule and try to account for various JWST scenarios.		Many projects have geared up for years with the idea of a decision being made on time. The potential expense to all those projects to maintain standing armies for 2 more years seems like it could kill important potential priorities. Prioritizing without the knowledge of JWST's results and without JWST's full cost seems very risky as well. Glad this isn't my decision.
48	Yes	People are already looking beyond JWST, we can continue doing so. There wouldn't be any data/results out of JWST 2 yrs from now anyway. As long as we are confident JWST will be finished and launched (I am), then we can just assume that in planning for the 2020s.		I see a big risk tying the DS to JWST launching. What if it is delayed another 6-12 months? or 18 months? Does the DS get pushed back 3 or 4 years? I don't think it's worth delaying the community's planning this much. There have been many new discoveries in the last decade in astro, and we should re-evaluate priorities now in response to those discoveries. What needs to be done in response to JWST discoveries won't be known until the mid 2020s anyway, after many JWST observations and the associated analysis. Delaying the DS that long is clearly overkill. I do understand that a 2020 report saying 'fund JWST' for the 3rd time would be a negative, but I doubt that is the report that would be produced.
49	Yes	We already had priorities. We have changed priorities. And launched on time or not, those priorities will change again. Trends in science are not always predicted so being flexible in being ready to change priorities is a necessary part of this game. You do the best you can at the time that you need to make a decision knowing that it will (and should) be open to mid-course corrections.		Ground-based priorities need to be made as well. Not all of astronomy depends on JWST. For instance, I don't think that the ELTs can wait another 2 years for NSF to make a commitment one way or the other. Will LIGO just sit back and watch foreign efforts take the lead, while the Decade Report gets delayed for JWST? They will immediately initiate a Special Review. Indeed, I heard that they were already thinking of doing just that in advance of the regularly scheduled DR. This process does not need chaos.
50	Yes	The science interests of the community are not hung on any one instrument.		Delaying the Decadal Survey on the basis of a single NASA mission is an inappropriate unilateral action in a joint process that has been important to our community for decades. In my opinion, the impacts to science and planning of uncertainty re: JWST are being overplayed by the NASA administration. While the thrust of this discussion is framed in terms of JWST, in reality what I believe is happening is the NASA is waiting to see if eventually Congress will kill WFIRST - its demise could then change the funding outlook for missions overall in the 2020s. If JWST runs into further delays and cost overruns, NASA may be asked to choose between it and WFIRST. Al very serious, but these are the normal slings and arrows of big projects, and NASA can deal with all that WITHOUT torquing the planning/development of ground-based facilities and other activities by delaying the Survey. If you continue to try and delay the survey, all of this will become public knowledge.

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51	Yes	While it would be a great loss, if something went wrong with Webb, the success or failure of JWST does not seem to be a significant driver of the science priorities for the next decade. If it works, there is no need to do it again, and if it doesn't there is no chance that it will be done again. Webb itself was designed for the science priorities of around the year 2000. The questions we have for the future are the same either way.		Even if delays eat into the budget of the next decade, NASA and the NSF will need direction on how to prioritize activities for the coming decade. Even just focusing on NASA, crucial investments in future missions will begin in parallel to the continuance of JWST and WFIRST. The time since the last decadal is already stretching long and its impact is waning. NASA is beginning to focus on future missions which were never considered in the last decadal; it is a good time for review. There are important areas of astronomy other than large space telescopes; I am concerned about significant loss of US leadership in these areas which has characterized the last decade. The US cannot assert leadership in any area until we have a decadal promoting renewed attention on astrophysical science. The only danger is in delaying the DS.
52	Yes	JWST has decimated the rest of astrophysics budget and opportunities for the last decade - I'm not interested in it doing the same for the future. The community can prioritize opportunities like the creation of a Probe line, Explorers, smallsat technology innovations, and even a flagship like one of the 4 being studied right now, in full honest acknowledgement that the timing and funding profile is driven by JWST and WFIRST but for which it is appropriate to invest in technologies to lead to one of the 4 flagships.		The disadvantage to delaying the DS is that it sends the message to the rest of the community that their priorities, are, again, not as important as JWST and that Astrophysics will continue to avoid having a balanced program because a single flagship will continue to be prioritized over other investments (Probes, tech, etc...). Also, it implies that the community cannot plan a future flagship even though they can, obviously, but with honest budget profiles that are realistic.
53	Yes	There is more to astrophysics than JWST and, without a clear understanding of when it will produce results that can be input to the decadal, we risk jeopardizing all the other science themes and missions.		See above.
54	Probably	We will have only a rough idea of JWST's science capabilities by late 2020.		Delaying 2 years would be catastrophic for ground-based astronomy; need a TMT decision and other issues. We also will now know much about JWST in early 2021 and will not know its lifetime or science legacy. I suggest having a mid-decadal that can re-evaluate the space science astrophysics landscape if needed.
55	Yes	It seems that we should do long term planning independently of whether JWST launches and deploys successfully or not. If it were a bust, none of the "surveyor" class missions that would be judged has really overlapping capabilities, and it is questionable (in any case) whether we would want a repeat of a JWST-like telescope. Were the launch successful we would just have lost 2 years, and there is no guarantee that JWST will launch on that timescale either. If there is a catastrophic failure of JWST the proper procedure would be to convene a committee to see whether the priorities of the Decadal need to be revised accordingly, rather than postponing the entire Decadal process for space and ground based astronomy.		We would be setting a terrible precedent, by which delays to a single facility stop the process for the entire US effort. We would lose credibility. We would lose the momentum that has been gathering in terms of planning and advocacy. And eventually all of that will have an impact on US leadership. The Decadal is a planning exercise: by definition one does not need to know everything in order to do planning. Unknowns are also part of a good planning process.
56	Yes			A delay would set a terrible precedent, and leave our community without a clear vision. Its not even clear what the delay is predicated upon - successful launch of JWST and passage through IOC? Observatory functionality within some tolerance of the requirements? Demonstrated groundbreaking "transformative" science? Why not ask the Decadal committee to take this into consideration, but outline a path forward that includes this as a possibility? Or simply just assume JWST will operate normally (albeit with a ~1-2 year delay). If something comes off the rails, the mid-term decadal could address the priorities outlined in the report in light of the new events, perhaps with a bit of guidance from the original committee (in the 2020 report).
57	No	JWST would take over as highest priority which eliminates all other possibilities		Advantage wins over any disadvantages
58	Probably not	Much like the last decadal, it could add significant uncertainty to science and funding situation for any large space missions.		There are both pros and cons to a delay. The obvious reason to delay is because of the uncertainty that a major JWST delay or catastrophic event adds to any science program planing for any future space missions, which could quickly make any plans obsolete. However, the A&A Decadal is also critical to the NSF planning process, and a delay would effectively cause NSF Astronomy and Physics to go into a holding pattern that indirectly costs significant intellectual and financial resources. I think its a priority to keep any delay to the Decadal planning process to a minimum (<1 year), and for a longer delay, a decoupling of the NASA and NSF decadal processes should be considered.
59	Probably not	JWST is a very complex instrument which should now be very clear to everyone. It has had huge cost overruns from the original proposal of < 1 billion\$. Future astrophysics space missions now under consideration are equally complex and more expensive. JWST's success, failure, or limited success will determine realistic goals for the future.		We now know what is possible from the ground. JWST will demonstrate what is realistic from space with current technology and space vehicles. No harm in waiting a couple of years.

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60	No	The JWST technology demonstration will directly affect the assessment of feasibility for future flagships, not to mention the budget.		It seems necessary to delay, although I suppose it could be possible to consider two possibilities (assume JW works, or assume it doesn't).
61	No	JWST takes up a major fraction of the budget. So its status is a major source of uncertainty until its performance is secured.		Advantages to delaying: * exoplanet community will have the best atmospheric characterization targets, through TESS. * status of JWST will clarify ability to allocate funds towards other projects. (Perhaps smaller ones).
62	Yes	There are always delays (and failures) for all missions. You can't sit and wait for them. The decadal survey should use the knowledge we have right now to scope out future directions.		
63	Yes	There are a lot of exciting things in the offing, even science unrelated directly to the IR.		Delaying the survey would be a big mistake. NSF needs it, the survey could be nimble in the event of any (even bad) contingencies, and the survey can be visionary and exciting under any likely JWST cloud. Moreover, SMD and the NRC must do its decadal in order and a delay could introduce a train wreck. Foremost, we must not introduce a dubious precedent, and must not compromise the decadal aura in the eyes of any of its major stakeholders, most of whom are not NASA.
64	Yes	Astrophysics is an extremely broad topic – one of its strengths as a discipline – and all the specialties within astrophysics (especially theoretical astrophysics) should not be beholden to the schedule of a telescope, no matter how great said telescope will be.		Astrophysics is not JWST. JWST is a telescope. It makes no sense to delay community support for countless scientific priorities based on a single telescope mission.
65	Yes	Science is not a linear single-file process, so there is a lot more to decide than the portions directly affected by JWST. The specific performance level of JWST is not critical for the survey. Critical to know for the survey is whether there is the extremely unlikely failure on launch or major malfunction on orbit, and that will be known before the survey wraps up per current projections. Scientists and leaders can operate with that uncertainty, given the lopsided likelihood.		Why slipping is a bad idea: 1. Leads to loss of momentum, loss of credibility with Government/Congress 2. Leads to gap in priorities (space and ground) 3. Rewards the institutions that failed to deliver on their promises (cost and schedule) by allowing them to dictate the community's calendar 4. Dilutes and hurts current efforts aimed at presenting cases to the Survey by making their science stale or forcing them to spend more resources to refresh 5. JWST is just an observatory, and the Survey is the whole program and future
66	Yes	We have a good sense of the major science drivers		Slippage will have a cascade effect on all 2010 priorities and future planning
67	Yes	We definitely can express our priorities well before JWST. We need to assume that all will work well with JWST and thus make the decisions of what are the next highest priorities. No problem in doing so.		A delay will cause in delaying advancements in science, loss of momentum, falling behind other countries.
68	Yes	It should be possible to consider both scenarios: priorities with and without JWST		It is not clear JWST will actually be launched in 2 years, so a delay may be useless. Delaying means ground based astronomy and theory will lose opportunities, and puts the NSF in a very difficult position of having to wait for decadal recommendations.
69	Yes	The effect of JWST on the next decadal survey should be minimal. IF the mission fails, the effects will be dramatic on NASA and beyond the capability of the decadal survey to adjust. IF JWST is successful, the impact of its science on the field will takes years to digest and, baring some spectacular unexpected discovery, will not impact the science of the decadal. Delaying the Decadal will set the whole program back by 2-4 years significantly effecting the overall program; if schedules hold, there will be room in the budget for a new major program or probe in 2024 which needs decadal recommendation by 2021 to get into the budget. In addition, a delay will effect NSF and DOE in unexpected ways. I thus strongly oppose delaying the decadal and see no benefit in doing so.		
70	Probably	In my opinion, JWST is not likely to produce results, especially early results, that drastically change the scientific landscape. The science results will undoubtedly be impressive, but probably not unexpected given the progress made with other instruments on ground and in space in the 20+ years JWST has been under development. In many ways, it is similar to the LHC and the Higgs, which is an incredible feat of engineering but is perhaps not as scientifically impactful as hoped. I think we know enough now to identify and prioritize a suite of missions with a range of timelines and costs. The only potential impact I could see on the 2020 Decadal is if JWST suffers some significant post-launch anomaly. This would lead to skepticism that NASA (and its contractors) can deliver these mega-projects and would place future mega-projects like LUVOR at a tremendous disadvantage, with some justification.		The real problem with this DS is that the amount of resources available to prioritize is smaller than in years past, largely due to cost and schedule growth of the top-ranked missions from the previous two DS. Delaying the DS only helps this if we are permanently shifting it (e.g. 2022, 2032, etc.). First off, I think this sets a bad precedent and we could easily end up with 15 year or 20 year planning. I personally believe that the astrophysics community would be better served by strongly encouraging NASA to complete WFIRST within its current budget, even if that means descopeing or canceling, and prioritizing one or more probe-class missions to get started in the 2020s and launch perhaps by late 2020s or early 2030s. The 2030 decadal survey can then pick up some of the more ambitious projects.

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71	Probably not	The operational status of JWST will not affect which science goals are seen as most important. However, JWST will not equally impact all of these science goals; it will profoundly affect our ability to make progress on some of them, whilst having much less impact on others. For example, JWST will have a paradigm shifting impact on z>6 galaxies, whilst having no impact at all on e.g. CMB and primordial Hydrogen studies. Therefore, not knowing the operational status of JWST will make it much harder for the decadal review to prioritize science goals, and to make recommendations for facilities on 5+ year timescales.		I believe that an approximately ten year cadence is optimal for a major review of the US astrophysics program. However, I do not see any substantial downsides to a two year delay. I would however not advocate for a delay of significantly more than two years.
72	Probably not	Extragalactic science depends heavily on JWST and represents a very large component of American astrophysics. A major shortfall in JWST's capabilities will spark an urgent effort to compensate, potentially with smaller NASA missions with faster turnaround. A functional loss of JWST might be mitigated with a major investment in several probe or discovery class missions to replace some of its capabilities on a much faster timescale than a replacement flagship mission. This would be a fundamental direction for the Decadal Survey to study.		<p>Advantages:</p> <p>A delay will bring clarity on JWST's status.</p> <p>By 2022 there will be more momentum and clarity on the status of WFIRST, a top priority identified by the 2010 Decadal Survey. WFIRST has undergone radical changes including a change of telescope and the addition and later descoping of a coronagraphic component, and is likely to suffer delays following on from JWST. Further descoping is possible.</p> <p>TMT and GMT are in a state of high uncertainty. This may or may not be resolved by 2020, but I think that recommendations from a Decadal Survey in that year will be too late to significantly affect their construction and first-light instrumentation. By 2022 their status, and therefore the state of ground-based observatories into the 2020s and 2030s, will be far clearer. This will enable the Decadal Survey to look for ways to complement the capabilities of these giant telescopes and enable transformative science.</p> <p>Disadvantages:</p> <p>Delaying will always bring clarity, so this could come up again. I believe that it is a bad precedent to set. The question is whether the current state of uncertainty is sufficiently exceptional to justify a delay.</p> <p>With WFIRST in a state of such uncertainty and possible descoping, it could probably benefit from a reevaluation. I see benefits for WFIRST both with delaying the survey and with keeping it in 2020.</p> <p>Delaying may sap some momentum from flagship mission concepts being studied, but I believe that this also has advantages. I think that the community may try to rally behind a mission that is too big and that eats all of NASA astrophysics for a decade or more. I think we could all benefit from an extra two years of reflection.</p>
73	Yes	Whether JWST is successful or not would not change the aspirations for what we need to do in the next decade		Delaying does not change the landscape for what the community would like to do in any significant way over the course of a couple of years. There is no significant technology jump a year or two away, nor any significant new paradigm that would be achieved with a delay of a few years.
74	No	Too uncertain		Could be meaningless
75	Yes	The astronomical community is bigger than just JWST.		
76	Yes	<p>While JWST will provide a major leap forward in many areas of astrophysics through its access to optical/NIR/MIR wavelengths from space, there is a large (perhaps even majority) of the astrophysical community whose science does not solely rely on the its launch. Consequently, those communities are sitting at the ready to begin the DS prioritization process now, and may be severely impacted by such a delay. Further, it is unclear to me how the launch of JWST, and initial science that will be returned ahead of a delayed DS, will have a major impact on the major science goals for the next decade.</p> <p>Just to be clear, I will be a very active JWST user once launched, and had a proposal ready to submit prior to the delay. My comments consider how to keep JWST in a positive light and ensure it achieves the highest possible impact given its current track record.</p>		I cannot think of any disadvantages by keeping the DS on the current schedule. On the other hand, I strongly believe that there are significant dangers in delaying by 2 years. First, by allowing JWST to continue to hold the rest of astrophysics hostage will undoubtedly increase any discontent that the community has for this mission. For instance, the bulk of the astrophysical community relies on the regularity of this report in their long-term funding and planning, thus any disruptions could cause significant hardships for many groups. It is also true that there are a number of new science questions and technology challenges that require immediate attention and would be severely hampered by such a delay. This is especially true in light of the detection of gravity waves in the last decade. Finally, by delaying I do believe that we additionally run the risk of falling behind as an international leader in various areas of astrophysics as other communities look to our DS for their long-term vision as well.

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77	Yes			Many scientific communities rely on the regularity of the Decadal Survey in their long-term funding and planning. A disruption could cause significant hardship. JWST is one of multiple areas of planning in the Decadal Survey, and should not impact our 2020 timelines.
78	No			
79	Yes	JWST is only one among many planned ground and space facilities. The DS should go ahead assuming a positive outcome for JWST. Should it turn out otherwise, NASA will need to consider how to salvage the mission. That could take years. Assuming NASA can come up with a credible recovery plan, that will need to be weighed against the new initiatives from the DS.		JWST launch in May 2020 is not assured. A year ago, the launch was 17 months away. Today it is 2 years away. We cannot delay progress in US astrophysics because of one poorly executed program. The world of astrophysics marches on with or without the US (e.g., ELT, SKA etc).
80	Yes			
81	Yes	While JWST is important, the astronomical community has many other facilities and goals to consider. Delays in one program should not be allowed to divert the community from the decadal ritual of taking stock.		The field is moving quickly, new opportunities (large optical surveys, gravitational-wave astronomy, etc.) and the political winds are gusty. All these deserve attention.
82	Yes	failure is not an option! Seriously, the entire SMD [astrophysics] enterprise will be under such duress with a failure that the future funding profile will be dim. Furthermore, the any exciting discoveries that go beyond what is expected won't come for years after launch.		The advantage is it looks like we're being cautious. The disadvantage is this put us out of sync with NSF or delays crucial decisions on the NSF side.
83	Yes	Regardless of the success or failure of JWST, the next priority will be in another scientific arena.		We already do things too slowly. While the next flagship may not see any change in eventual launch date due to a decadal slip, the moving forward on smaller class missions should be accelerated, and not delayed. If the 2019 budget does not include WFIRST, as requested by the white house, then the need to hold the next decadal sooner rather than later is even more critical.
84	Yes	Even if JWST explodes I don't think that should change our priorities – it's not like there is a JWST successor in the mix.		The decadal preparation already has taken/will take a lot of time from people – I think delaying it just adds to that.
85	Yes	The launch of JWST is in reach and within the first half of the decade it won't be too hard to predict in which direction research will go. One could develop a scenario where JWST launches as currently scheduled and a second scenario where JWST is further delayed or some other event occurs that disrupts the JWST mission. On the other hand other space missions as well as ground based astronomy are not heavily affected by the fate of JWST, especially in the age of time domain, multi-messenger astronomy with e.g. gravitational wave detections and follow-up and fast radio bursts.		The slipping of the decadal survey would set a new precedence that a single big project can derail efforts of the entire US astronomical community. A lot of initiatives and preparation has already gone into the anticipation for the decadal survey process to commence at its expected time. Furthermore, a lot of funding decisions depend on the outcome of the decadal survey and thus gaps in funding or effort could occur that would derail efforts to prioritize ground-based facilities in the context of scientific discoveries that have emerged over the past decade, gravitational waves, the claim of a detection of a signature of the cosmological dark ages, localization of fast radio bursts, an explosion in the number of exoplanets discovered, etc. Only a small fraction of this science would be affected by JWST. Any delay in the decadal survey could cause gaps in funding and research that might be hard to close in the future and will delay a reassessment of research priorities potentially putting the US astronomical community at a disadvantage.
86	Yes			Early planning for the next large mission is essential if such as mission is to be made ready for launch in the 2030s. In the (hopefully unlikely) event of JWST failure, the nature of the next large mission can be adjusted to fill the void. It is clear that facilities for the next decades will involve on-orbit assembly of large structures. Independent of the ultimate flagship mission choice, infrastructure for tele-robotic, on-orbit assembly, verification, and repair will be needed as the complexity of future flagship missions grow. The largest risk to JWST is the current lack of this capability. A delay in the DS will harm the credibility of our field and would show a lack of leadership in the US investment in astronomy and related sciences.
87	Yes	The decadal survey will establish priorities regarding missions that will likely be launched in the mid-late 2020's or early 2030's. I do not think it is reasonable that JWST would delay its launch long enough as to affect the way we currently think about missions in this timeframe. It would be pretty bad if it did.		In light of NCOA's recent announcement that it will pursue a significant (20-25% level) involvement on both TMT and GMT, in order to provide access to 30m class telescopes on both hemispheres for the US astronomical community, it is imperative that the decadal survey is not delayed. A delay in the decadal survey could translate into a delay in a potential NSF financial commitment to these two projects, increasing the financial risk for both projects, and jeopardizing the possibility of US access to 30m class telescopes in the next decade.
88	Yes			A delay in the decadal process will delay work on finding public funding for 30-m class telescopes in the US, jeopardizing US leadership on large aperture projects. Many projects have already been delayed or cancelled due to JWST. It's best we don't allow that trend to continue.

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89	Probably			I understand that NCOA will advocate for a 25% public share of the two U.S. ELTs. If the Decadal Survey agrees that this is a priority, its endorsement could prove critical to the success or failure of either or both projects. Both are in a critical phase right now where injection of 25% of their capital costs would likely ensure their completion. The longer it takes to receive that assurance, the greater the chance that the projects could fail, leaving the U.S. without an ELT. The European community would dominate faint, ground-based astronomy for the foreseeable future. Delaying the Decadal Survey is dangerous for this reason.
90	Yes	Rebuilding JWST if something goes wrong is not an option, so either way new priorities will need to be made.		Delaying makes the process look more capricious and less serious
91	No	The funding uncertainty will be too large.		If the survey is held on schedule, the community will have to spend time developing multiple scenarios for space priorities, based on multiple JWST possibilities.
92	Yes	Unless it is cancelled (which should be clear in the next few months) we should assume it will successfully launch and become operational. At this point we don't know how long the launch will be delayed, there might be other delays that push the launch into 2021 or beyond, which would push the decadal toward mid-decade. The 2010 report is too dated and needs to be updated quickly. There are many priorities that don't depend on JWST being operational.		If JWST fails or doesn't launch, an update to the DS could be done. It would be a major undertaking to recover from and replan NASA priorities if that were to happen. It would be a much bigger effort than just another DS, more community input and planning would be necessary. During that time, it would be better to be guided by the 2020 DS rather than the 2010 DS. Other astrophysics goals could then continue to be pursued rather than everything grinding to a halt.
93	Yes	there is no good reason why other investigations should not be reviewed and progress as appropriate. If some planned is contingent upon jwst, then that can be a component of the ranking.		There are always advantages and disadvantages to delays with regards to technology development, and the 10-year cadence is arbitrary. Projects that are ready now, or have been delayed (possibly because of JWST) should not be further delayed, especially if they are not contingent on JWST. We have enough to do now which are "good enough"; waiting for things to get "better" is more risky.
94	Probably	Usually by the time a mission is actually scheduled for launch, we already know pretty well what to expect from that mission in the way of a priori benefits. I don't think that having the launch done and over-with has any bearing on the planning for future missions, especially large-scale missions that require a lot of advance planning and advance recommendation to proceed. Hence, I believe that the next Decadal may proceed with the assumption that JWST will be launched in 2020 or 2021. The science from that mission would not yet be available in any case.		The advantage is to keep the planning phases on schedule for recommended upcoming missions despite JWST delays. Inasmuch as any further difficulties are not likely to result in terminating the JWST mission, we should continue to forge ahead with work and planning on future missions, and derive our recommendations on the basis of those new mission goals, setting priorities appropriately. The danger of putting off the survey until JWST is launched would be that further JWST delays would put off the survey even further, given the logic of the initial delay. Therefore I strongly support moving ahead with the current Decadal schedule.
95	No	After so many years of anticipation for JWST, it is hard to believe that the priorities set with the knowledge that JWST will perform as-designed, or alternatively knowing that it will not (unfortunately), would even be comparable. Even ground-based efforts, particularly sensitive IR spectrographs for follow-up observations, would be significantly affected by this. Although my opinion is primarily based on my experience within my own topic-of-interest (assembly of galaxies and black holes through cosmic epochs), I believe this would also be the case for the exo-planetary community.		Given the delay foreseen in JWST operations, the delays we've seen with implementing the different 30-meter-class telescopes, the very recent launch of TESS, and the still-tiny number of GW events (not to mention GW+EM), I actually think that delaying the DS by a couple of years carries very little danger.
96	No	We need to get it up there and make sure it works!		I don't see any harm in delaying the survey until JWST is up. The status of JWST is a hugely important piece of information and we're not going to suddenly be running around like chickens with our heads cut off once the current decadal "expires".
97	Probably not			
98	Yes	NASA has already decided that WFIRST is the next big mission after JWST. I think it would be unfortunate to keep stacking up large optical and infrared missions for decades into the future. The decadal survey can simply have a disclaimer for what to do if JWST doesn't work, and how that changes priorities. In reality, with WFIRST already in the queue, I don't think we should be doing another mission beyond the Probe class now anyway. Let's move back to a broad base of Probes and Explorers, and away from single missions that squeeze everything else out. If we take that viewpoint, then the decadal should happen as planned.		I think that, first and foremost, keeping the schedule sends the message that no mission is more important than all the rest of astrophysics, and that this is important. But also, there are other things that should probably be happening sooner, rather than later and delaying the decadal survey by two years almost certainly means delaying the implementation of most other future missions by two years. The LIGO discoveries in particular have changed the game in terms of what priorities should be going forward, in a similar way to how discoveries of exoplanets and the accelerating Universe were mid-cycle game-changers. Pushing ahead with a 10 year old plan of priorities for another two years when the 10 year old plan didn't anticipate what we now know means that we run the risk of investing deeply in things that are not too exciting.
99	No	JWST is eating into next decade's budget. JWST needs to launch safely before the next Decadal.		None. NSF should accommodate.
100	Yes			

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101	Probably not	I'm surprised that this question has to be asked. JWST's overruns in cost and schedule have paralyzed astronomy and continue to do so. Waiting in the wings is WFIRST, which won't launch until the mid- to late-2020's. Both the funding and the available time for the decade of the 2020's is largely spoken for already. If we want a Decadal Survey every decade and want for it to be useful (since a lot of people spend a great deal of time working on white papers, assembling and synthesizing inputs, etc.), we need to make sure that the previous decade's priorities have been completed. We haven't complete the highest recommendation of the 2000's decade yet, and that has delayed the highest recommendation of the 2010's. Holding a new Decadal Survey now should only happen ****if***** we admit our prior mistakes and re-prioritize for the 2020's afresh. That means putting WFIRST potentially on the chopping block to see if it still rises to the top. One of the problems with the last Decadal Survey was that NASA protected JWST so that the Panel had no say in whether it was still a priority. (I think it should have been axed at that stage.) As a result, the last Decadal Survey was, in my opinion, largely a farcical venture.		I see no disadvantage in delaying by two years. We have very little to gain by holding it now, given the uncertainty JWST has placed the entire astronomical community under.
102	No	JWST will be a paradigm shifting facility. With all due respect to the many scientists who have spent appreciable time predicting what JWST will find, we will have no idea of its impact on our perception of the universe until results start flowing from it. Regardless of whether these results exceed expectations or don't live up to them, not knowing what JWST can do before the decadal survey is completed guarantees that the decadal survey will be obsolete before any part of it can be implemented. Such a situation would render a disservice to the community, both because the agencies then no longer have a compelling guide for their future, and because the many astronomers who spend hours writing white papers and serving on panels will be effectively wasting their time.		Advantages of delay: the decadal survey report stays relevant, and is invigorated by initial JWST results. Rate of progress on technology needed for large and probe missions would provide a clearer discriminator for decadal panel recommendations. Future of WFIRST would be considerably clearer. Disadvantage: work done to date on large and probe missions would become stale, forcing additional agency investment to update them. No effect: Loss of US leadership in key areas. This has already happened in X-ray and gravitational wave astronomy, and, with the US not being part of SKA, radio astronomy in large measure.
103	Probably	Many areas of astrophysics are largely independent of JWST (although they will be impacted by JWST). The program for those missions can be planned without knowing how JWST fares.		I would vote for an earlier rather than later decadal survey. It will be important to plan powerful missions for the mid 20'ties to keep up the momentum in the high-energy, multi-messenger, and cosmology communities.
104	Yes			Delaying the DS only further delays non-JWST missions which might otherwise have a DS document which strongly argues for their importance.
105	Probably not	Despite the lofty goal of "prioritizing astrophysics space science goals for the next decade," it is inevitable that cost considerations will play a significant role in such a task. If it did not, there would be no costing element to the DS. There is nothing fundamentally wrong with including such considerations in evaluating the broad profile of science questions that confront us. In the face of this reality, a major uncertainty in the operational status of the JWST would significantly hamper the process.		I believe the advantages lie strictly in having better knowledge of the practical envelope for expenditures. This also affects momentum for 2010 DS priorities. Despite NASA's stated commitment to WFIRST, the recent cancellation attempt in the WH budget makes it clear that there may be additional battles on this front. I would expect more clarity on that issue once the (hopefully positive) status of JWST is known. With current knowledge, the DS could be forced with an internal struggle about whether to look beyond WFIRST to new science initiatives, or to double down on WFIRST science in possible anticipation of near-term threats. I am not in favor of delaying the DS simply because new science problems will present themselves by doing so. That logic holds every year. The JWST question is different, and tips the scale toward delaying the DS.
106	Yes	A lot of astronomy is not dependent on JWST's results.		Everything seems to be delayed anyway and top priority missions are launched a decade and a half after they have been ranked a top priority. Maybe it is time to skip one decadal to "catch up" or postpone it by five years?
107	Yes	It seems to me that the decade survey has to assume that JWST will be launched soon, and will start its pioneering observations of the Universe, as planned. The alternative, that the project is significantly delayed, should not affect the priorities of the Decadal survey for the future. If something catastrophic happens, and some kind of launch failure occurs, this would be a major perturbation to the next decade, but I think that the decadal survey, by its very nature, should be forward looking and positive.		Advantages include providing a positive impetus to new science initiative that have largely been on hold because of the huge cost of JWST. Young people, who hold the future of our field in their hands, may become discouraged, if the exciting new ideas keep being pushed further into the future. Our field lives and dies on new ideas and new blood. Already, new projects seem far into the future, and delaying the decadal survey will only help to push exciting new initiatives even further out. This is not good for our field. An example is the considerable momentum being built up, both from an instrumentation point of view, and from the stimulation of new science ideas, with the work being done by various STDTs on concepts such as the OST etc. These melting pots of great ideas for future observatories have had a very positive effect on a field that, for so many years, has been dominated by JWST. Any delay in the decadal survey, will adversely affect the aspirations of countless young people who are caught up in the excitement of such future potential observatories. I strongly feel we must stick to the current schedule.

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108	Yes	We can walk and chew gum at the same time, and not only can we, we must. Our community is large enough that the non-JWST related parts of it can keep the focus on the other astrophysics-related science domains, and determine where NASA should move forward. If we do not attempt to prioritize, we will lose momentum.		Loss of US leadership, impact on planned activities, momentum for particular science questions which depend on the new technology will be lost
109	Yes	Many missions and science goals will be considered besides those that require or would greatly benefit from IR-MIR spectroscopy.		One may always wait to get more information before planning new activities. We could start earlier on new projects that don't relate to JWST if the decadal review were to start on time. Many new instruments are have justifications that are independent of JWST's potential return.
110	Yes			Disadvantages: the NSF and DOE need our guidance sooner rather than later
111	Yes			The Decadal Survey includes much more than NASA missions. Delaying it would be highly detrimental to ground-based OIR astronomy and other fields.
112	Probably not			
113	Yes	The only thing that would substantially change planning is a total loss of JWST. While possible, the risk models suggest this is very unlikely.		The danger is that the decadal is for all of astronomy: many NASA science programs also require coordination with ground-based and workforce astronomy, and a delay of 2+ years in the decadal doesn't help that at all. The lack of knowledge about JWST's results is a reason to focus on a balanced plan for the future that can flexibly respond to future science developments, which should be the case no matter what.
114	Yes	The science goals themselves are obviously independent of \$. And they can change quickly as we learn more. While their order of funding priority does change, as long as scientists only advise NASA there is no reason to withhold current scientific advice.		Funding / implementation delays are the norm - if we delay advice we will not have a decadal process. Moreover, keeping up the science advice also keeps up the pressure, and this is the most important part of the equation. To delay advice signals accepting that there is no urgency.
115	Yes	Astrophysics is about much more than JWST. Since the last decadal, we have moved forward with ground-based facilities, Explorers, and new data from older missions, and we have powerful new facilities in construction which will operate regardless of JWST flies. We still need to be able to deal with time domain astronomy, gravitational waves, other elements of astrophysics which have made more progress since the last decadal than JWST has, and other parts of the spectrum where good science can be done but current instrumental limitations can be overcome. Just because JWST has sucked up a huge fraction of the new mission money and attention in space-based astronomy doesn't mean that we all have to be held hostage to it. We have a sense of what JWST will be able to find if it works, we have a sense of what WFIRST will do if it works, and we also know what ground-based facilities will be able to find. And what their shortcomings will be. Meanwhile, early career astronomers still need to be able to plan what they'll be doing in 10-20 years, and have some sense of whether they'll even still be able to stay in the field based on current trends.		JWST has put astronomy into a terrible crisis, and has cost us 8x-16x its original estimates, and has left the entire field hanging on its launch for results from a waveband that only covers ~1.7 dex in wavelength. A better-managed program could have had ~4 new flagship missions by now for the same cost, even with horrible cost overruns for all 4 missions. This is a disaster and a terrible embarrassment for astronomy in the face of the public trust even if JWST is a stunning success, and I don't think we'll be able to (as a field) grapple effectively with the consequences UNLESS this problem is still hanging over our heads as we deliberate in the decadal. We need to know that new missions can be killed without sinking the field, that they can fail without sinking the field, and that a variety of avenues of scientific inquiry have futures in astronomy. If we wait for the decadal until after JWST launches, we will make decadal decisions having learned the wrong lessons PARTICULARLY IF JWST IS A SUCCESS.
116	Yes	Whether such priorities will be accurate is a different question altogether, but with JWST we're still operating significantly on prioritization from the 2001 DS. We need to prioritize in light of the JWST situation rather than avoid prioritization until that situation is resolved. The head-in-the-sand approach keeps us from being wrong, but that doesn't make it right. Setting goals for the decade will still give direction to non-JWST funding.		The DS risks losing its relevance as a set of milestones to be accomplished in a decade, and that perspective shifts astrophysics from being driven toward a goal to meandering along a path. The DS might be more accurate if it waits, but I'd equate that to throwing a Frisbee and deciding who you threw it to once you've seen where it's going. A lot has changed in ten years, and it's time to update the priorities in light of developments over the past decade.
117	Yes	If JWST fails, there will not be JWST2 in the next decade; the four mission concepts that NASA is supporting, one of which would likely be the "flagship" mission of the next survey, have nothing to do with JWST capabilities.		The danger is simply that science marches on, and delay would significantly impact moving forward with new findings. Plus, and this may not be the right place to state this, but NSF has extreme immediate challenges that a timely survey would help address, and for this reason they do not want a delay.
118	Yes	Astrophysics is a broad field and if one assumes that JWST will achieve its stated goals, it should be possible to plan beyond it.		NASA is performing studies in preparation for the Decadal, for Large missions and Probe class missions. If the decadal is delayed, these reports will be out of date by the time they are reviewed by the decadal committee. Opportunities to propose for future probe-class missions would be delayed as well, and this may impact the cadence for explorer class missions as well. Multi messenger astrophysics is a new, important, and rapidly growing field that needs to be prioritized in the decadal survey. If the US waits, other countries, will take the lead, making it more difficult for our scientists, especially young up and comers, to continue to be funded.
119	Yes	I think that subfields that will not primarily use JWST should not have to wait until JWST launches in order to have an opportunity to present their science goals and mission proposals.		I see a danger in scientifically unnecessary delays. We want to have missions built and launched while there is still current expertise around, and we don't want the scientific goals don't lose momentum in the community due to a time gap in the available instrumentation.

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120	Yes	I dont think the partial or, God forbid, complete failure of JWST to meet requirements will change the emerging science priorities for the next decade which is what I strongly believe the Decadal is really about. It might change NASA's implementation model. For example, if the Decadal recommends a Probe line for Astrophysics then the probes selected for implementation may well be influenced by science gains or losses associated with JWST performance		If we wait another two years, its just that much longer NASA astrophysics remains in the JWST desert operating against a Decadal that is now 12 years out of date. I think NASA needs to assume success of JWST, press forward with the Decadal and adapt if things dont play out as planned with JWST.
121	Yes	Every decadal looks at what's available at the time; although any new mission might provide insights (operational or scientific) that would change the assessment. Is it possible to prioritize goal before IXPE is launched? Or before LSST comes online? Or before the HST mission ends? The answer is "yes" because we always aim to make the best judgment based on the knowledge we might have now. Granted, JWST is a big mission but it's far from the only facility to do astrophysics. Assuming JWST's successful launch, it will takes years to fully grasp what the impact of JWST's results will be on the big science questions anyway.		The community is very much in proposal mode, racing to mature technologies in time for the Decadal.; long-term goals have been pushed back "to get this done first", e.g. Chandra had a special call to observe targets, the NASA technology development grant have a schedule to accomodate the known decadal date etc. Lab organizations and hiring committees make decisions based on "getting ready for the decadal in 2020", and departments fund extra development activities from limited funds such as endowments expected a thumbs up or thumbs down for that technology in 2020. They might not be able to sustain that until 2022. No one of these could not be adjusted for a later decadal, but in the sum it creates significant unrest and planning uncertainty in the community that it might impact our ability to adequately prepare for a decadal in 2022 (or if JWST slips again, 2023 or 2024).
122	No	The delay maybe considerably longer than expected and will have a domino effect on the funding timeline available for WFIRST. Therefore we'll once again have a decadal survey without funding for the proposed missions.		I will support a 2 years delay, as there will be improved science goals and improved technology.
123	Yes	It is unclear why the exact JWST launch date (May 2020 or some time later) should make any difference to the long-term planning and prioritization that is achieved by the Decadal Survey. The DS is about much more than JWST, and while we all hope that JWST suffers no further delays, life carries on for the rest of us.		Delaying the DS has associated risks. If we take the view "let's wait until JWST is sorted out before planning anything else" we could be waiting an unknown amount of time.
124	Probably not			
125	Yes	Science problems to be addressed by JWST will not go away just because JWST is or is not operational.		JWST has cost too much and been delayed too long to additionally impact the DS. The community and NASA need to stop aiming for such complicated, expensive hardware. The overwhelming success of Kepler speaks directly to this problem. There could well have been 6-10 successful missions with the funds taken by JWST - how far ahead would the field now be with those 6-10 missions? Do the DS on schedule!
126	Yes	If I think about the science I think is important for the future, the answer is the same whether or not JWST is successful.		If we delay the Decadal survey, then the ground-based projects will suffer from lack of community input. If we delay the Decadal survey, then some missions (e.g. WFIRST) may be too mature to de-prioritize. (Whether that is a pro or con depends on who you are).
127	Yes	JWST is certainly important, but there is always uncertainty in the future of instruments. Space based instruments can be lost at launch or at any time during the mission. Even with the not delayed launch date, the commissioning and verification of JWST would lag to the end of the DS, so the results could not inform most of the discussion and debate of priorities.		I see a great danger in delay. DS is important for the broad field. It is a big deal and it makes sense to keep it on schedule.
128	Yes	Whether JWST works or not, there are many issues that must be grappled with within space science. How these are handled does not hinge on the success or failure of JWST		New science opportunities have opened and the DS must respond to these. Delaying 2 years threatens loss of leadership in crucial areas, as well as loss of momentum from current plans
129	Yes	There is always risk and the potential for delay in every mission. One just has to factor this into every Decadal Survey.		The JWST is only one mission of many. It makes no sense to delay the Decadal Survey (DS) because one mission has slipped. The DS cadence is used for the planning of other missions and delaying the DS will have a negative impact on other missions.
130	Yes	There are always missions at various stages of development at the time of the decadal survey, and the funding that will be available over the next decade is always uncertain. The decadal survey should and will prepare 'menus' of options for several potential funding scenarios.		I oppose a delay that could inadvertently undermine the future of ground-based OIR astronomy in the U.S. A concerted effort is underway to seek critically needed funding for the GMT and TMT projects as part of the decadal survey. A delay in the survey will delay potential funding, which might well jeopardize both projects. Given the European investment in E-ELT, we risk a future in which U.S. ground-based facilities become virtually irrelevant over a broad range of science cases.
131	Yes	Science needs to move forward not contingent on a single mission, and it can move forward by utilizing both space-based and ground-based instruments across the broadband EM spectrum. There are other missions and observatories development that are parallel to JWST and are not highly dependent on the outcome of JWST.		There is a multitude of science frontiers and technology development that need to move forward. For example, the most recent multi-messenger astronomy development in gravitational waves and neutrino astronomy, connecting with the EM observations of astrophysical sources. We should not delay the decadal to ensure we remain at the forefront of these latest development in all sides.

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132	Yes	JWST will obviously not be able to cover all aspects of astrophysics, and it's not clear that if and when operations commence there will be any causal relation to missions selected for science operations in the 2030s. Missions for the 2020 Decadal should be judged on their own merits without reliance on what science JWST may or may not do, especially since there is not a guarantee at this point that JWST will be a success.		Tying the funding, direction, and state of astrophysics to the status of a single mission is incredibly risky. Surely this should be a lesson learned from the 2001 Decadal Survey and JWST mission itself. Delaying the Decadal because of the uncertain status of JWST is running away from the problem and hoping it eventually fixes itself instead of addressing the problem that resulted in the massive cost overruns and delays that JWST has experienced. The rest of astrophysics has suffered from it as well, and postponing the Decadal will only exacerbate the cost on astrophysics as a whole. There are certainly a wide range of worthy concepts to be considered and several interesting questions in astrophysics to explore, and it might be a better idea to diversify the mission portfolio with smaller missions (i.e. < \$1 billion) rather than have astrophysics live or die on (more or less) a single mission. Add to that the public relations disaster that could be created by delaying the Decadal because of JWST. Most of astrophysics funding ultimately comes from public sources, and the appearance that JWST is adversely affecting all of astrophysics may adversely affect the public's willingness to continue funding such endeavors.
133	Yes	The decadal survey should set the science priorities for the upcoming decade, without addressing the finer details of funding availability. I think this can still be achieved on the current timescale since the field needs to start addressing the science objectives beyond JWST. NASA can then be left with the responsibility of delaying upcoming missions to achieve those objectives based upon available funding.		The danger is JWST holding up future progress in Astrophysics research
134	Yes	I think it is very unlikely that JWST will be so revolutionary right out of the box that space science goals would be changed.		Astrophysics is not just OIR science even within NASA, and of course the NSF and DOE need to have their decadal priorities sorted out. If we have so little confidence in the success of JWST completion and launch that we wish to delay the decadal because we think that it won't work we are in worse trouble than I think. And what if it slips again, which is possible, unfortunately. I would like to have somebody explain to me the rationale for this proposed delay.
135	Yes	It's urgent to get moving on future plans, even if we can't fund them until after the JWST launch. In particular, we need to prioritize technology development based on Decadal Assessment of Science Goals. I think there is, and will be, high confidence in the success and in the initial return from JWST, no need to wait for the actuals.		Advantages only. Planning is essential, we need to try to catch up from the "slip" of ten years caused by JWST delays. We need to motivate young scientists to enter the field and work on the projects for the 30's and 40's. Delay of the decadal will cause an inevitable exit from astrophysics for some.
136	Yes	I do not see why the JWST launch date would affect prioritization of future science. I suppose that things would change if JWST blows up on launch. Other than that, the prioritization is the same. NASA can then make decisions about how to implement the prioritization for space, based on budgetary constraints that we will only know when JWST is more settled. However, the size of budget available should be (mostly) independent of prioritization.		The ground-based Decadal Survey must go forward as planned. Dividing the Decadal Survey into ground-based and space-based undermines the entire process and will weaken the results. The decadal survey should be a coordinated effort to plan future science, rather than a specific focus on ground or space.
137	No			
138	No	JWST will inform future science		We would be planning on incomplete data
139	Yes	There are many other astrophysics programs besides JWST.		Fields farther removed from JWST science are counting on an on-schedule Decadal Survey.
140	Probably			The biggest factor I see is the opportunity to roll the dice on a chance of administration in 2020. A survey with a new administration might see things very differently than they look now
141	Yes	JWST is one piece of the astrophysics and astronomy picture. One that is expensive, delayed, and potentially at further risk. Why let its problems drag the rest of the field down? Particle physics is more than the LHC (or the SSC) and astronomy is more than a single telescope.		Science needs to move forward, stick with the schedule, and do what we can with the decadal survey. There might be uncertainties about JWST but those don't need to color our entire field.
142	Yes	There are many experiments that are completely independent of JWST that need to be discussed sooner rather than later. For example, it is past time to discuss the next decade in gamma-ray physics.		
143	Yes			
144	Yes	Many exciting discoveries can be made w smaller missions. Look at IUE, and now TESS. We can't lose momentum.		We should not delay the decadal survey. We will lose too much momentum and hurt future 2030 missions such as Athena ie Chandra/XMM replacements
145	Yes	JWST capabilities (instruments, wavelength, sensitivity) are already frozen, so the current uncertainty in JWST schedule has no significant impact on future science priorities planning.		Dangers: Loss of momentum and loss of precious time to plan future missions. It would also set a dangerous precedent that could disrupt future decadal. Delaying the DS to wait for JWST operation would be somewhat open-ended: what happens if future delays, or if problems during on-orbit instrument verifications.
146				Unknown impact on timeline of already planned activity. The timeline of JWSTs launch informs where the interesting science will be at the time next priorities will be off the ground.

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147	Yes	While JWST will be an important flagship instrument, the astrophysical themes of the next decade are not exclusive to it (e.g. gravitational waves). Voice support for JWST in the decadal and presume launch success in the planning process.		Delays in DS2020 have many negatives - impact on 30m class ground based instruments, impact on proposed mid-scale and large facilities, etc. There are many other communities who rely on the regularity of the decadal reports in their long-term planning (and funding), and such a disruption in this process could cause significant hardship and loss of US leadership.
148	Yes	<p>It is the sign of a healthy community that there are always missions in development. Stopping the decadal because of JWST prioritizes one field in an unfair way that is not proportional to the scientific importance of that field (note: the size of a community is more a sign of its "sexiness", not of the real scientific importance). A decadal is a snap-shot of the state of the community's priorities. Changing the schedule would effectively mean that JWST's technical problems and cost overruns dictate the progress in all of astronomy that is not OIR-astronomy. This is not how a fact based approach to setting priorities should happen - and JWST not being done in time again also means something about the technical and managerial capabilities of this subfield of astronomy. Why should we hide that fact?</p> <p>I would just like to note that the 1990s decadal wasn't delayed by the launch of HST, and neither was the 2000 decadal delayed because of the launch of Chandra.</p>		<p>Delaying the decadal means that the US will lose momentum in all other fields. Examples include the further development in X-ray astronomy, where a possible mission will have to be complementary to ESA's Athena, decisions about the size of a possible US contribution to LISA, and many of the developments for smaller missions that leverage upon ground based developments. Example science questions are: fast multiwavelength follow up for LSST transients? Gamma-ray observations to leverage upon CTA? What do we do about the loss of any UV capabilities? How will the US react to the developments of the European ELT? How will SKA shape radio astronomy?</p> <p>All of these questions are very important and do not hinge on JWST, and not being able to answer them now will put the US at a real disadvantage in the long run.</p>
149	Yes	We cannot wait for JWST's launch to work on our community priorities. That attributes undue importance to a single mission and cripples our burgeoning probe-class mission development. The former has been an enormous problem in the last decade and should not be allowed to "creep" into the next one. The latter was a major priority of the last decadal and we should work as hard as we can to insure the success of our probe development, as our community's health and optimism will depend on these probes if there are problems with JWST's launch or operations.		We have a decadal for a reason: to evaluate our science priorities as a community every 10 years. If we sacrifice this important activity to a single mission, we will potentially lose on all of the fronts mentioned above: loss of momentum, missed opportunities to capture the moment of discovery in gravitational wave science, compromise US leadership, and damage probe and other mission/technology development already in progress. We will also increase resistance by scientists and government to future "great observatory" class missions because their schedules will be perceived to cease our ability to plan and prioritize as a community. A delay would be a terrible idea. If we need an interim review in 2-5 years based on JWST's operational status, we can shoulder that extra work, but we should not place all of our science on hold for a single mission, not now and not in the future.
150	Yes	Certainly, JWST will make great discoveries once operational. But will a delay in its being operational (and associated delay in those discoveries) dramatically change the priorities of future space missions, or merely delay their implementation because of the necessary diversion of resources to keep the JWST project running prior to launch? I suspect the latter, in which case I see no reason to delay the decadal review for everything else.		A delay in the decadal review would result in loss of momentum for the priorities identified by Astro2010, and impacts the timely prioritization of other areas of astrophysics. As an example, the detection of gravitational waves in this decade demands an immediate and coordinated approach from the community moving forward, which can only be provided by the upcoming decadal review. The US will lose its position of leadership in these areas if the decadal review is delayed.
151	Yes	Either JWST works or it doesn't. If it works as expected it will still take years to appreciate the impact. I don't think we can delay the Survey long enough to absorb the news and its implications. If it doesn't work as expected then it will certainly take a long time to find out what happened and to develop options. I think we need to know where our community stands either way.		I think we need to maintain momentum for doing what we said we wanted to do. Paul Hertz has supported four great observatory studies and nearly a dozen Probe mission studies. I recommend that we read the reports, choose the top priority science topics based on what we already know, and continue full speed ahead. None of the new mission concepts will be so fully developed that we truly know cost information anyway. But if we know which ones are top candidates we can concentrate on those. Priorities for ground-based astronomy also need to be evaluated sooner rather than later.
152	Yes	Space astrophysics is not solely the province of JWST.		The decadal is reviewing all of astrophysics, not just space-based IR astrophysics. The rest of astrophysics doesn't come to a screeching halt just because of the JWST launch slip. There's no guarantee that JWST will actually launch in May 2020 -- does the decadal just keep getting pushed back indefinitely?
153	Yes	A healthy astrophysics community will always have a variety of programs at different stages of development. Presupposing that one must wait for JWST is in a very real sense predetermining the priorities of the Decadal. It is essentially saying that radio, optical, UV, X-ray are all secondary concerns to space-based infra-red. The 1991 Decadal was released Feb. 1, 1991, after a Hubble launch in April 1990. The early results of Hubble were not known at the time of the bulk of the writing, and certainly the COSTAR fix was not yet in the cards. But the Decadal went ahead anyhow. The 2000 Decadal was released very shortly after the launch of Chandra and XMM-Newton, so results from those missions were not yet incorporated into the planning. The situation with JWST although perhaps extreme is not entirely new. Why should this time be treated differently? JWST has already dominated the 2000 and 2010 Decadal reports. It is time to continue with the regular process of periodic reports. If JWST cannot fly in time for consideration of its results in the 2020 Decadal, that is already an important data point about the ability of the space-based IR community to complete projects, which needs to be considered for the upcoming Decadal rather than hidden or explicitly accommodated.		This is a very bad precedent to set, to delay a report by 20%-25% in terms of time, simply because a single mission, however expensive, has yet to launch. There will always, always be missions that have not yet begun to operate. There will be missions that fail. Astronomy needs to keep moving forward, with a regular cadence, and with a unified voice that says that we are more than one mission. We have a broad set of ideas, and we will be bringing more than one forward at any given time.

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154	Yes			
	No	See below.		<p>I'm not sure this is asking the correct question. I believe the correct question is "does deciding priorities every ten years make sense given the current development cycles of major astronomical projects, and the rate of change within the field?". The JWST situation is just the most visible symptom of the problem.</p> <p>To name just three easy examples, JWST, WFIRST, and LSST will not become operational until well after the decades for which they were recommended. It is not uncommon for projects of this scale to stretch to 20+ years of development. Given the lack of funding in the field, the end result is a queuing effect that literally locks out new development for timescales approaching a typical astronomer's professional lifetime. I could name half a dozen talented people who left astronomy as a result of this.</p> <p>In turn this queuing effect this makes the decadal survey unresponsive to sudden changes within the field. The most obvious being the explosion of exoplanet research in the last decade, and the sudden turnover of gravity wave science from being something of a crackpot field to legitimacy in just a couple years time. Will these fields have to wait in line behind others that have been waiting for years for a new major project?</p> <p>While I feel mildly guilty about not supplying any solution to these problems, I will respond that it's vastly beyond the scope of this letter!</p> <p>In regards to the actual question asked: it's pointless to hold the decadal survey since nothing it recommends can be implemented in the next decade due to the queued backlog of projects. However, it would be worthwhile to examine the state fo the field anyway.</p>
155	Yes	We have good estimates and ideas of what JWST can achieve. We'd only be missing the surprises.		Delaying will lose the momentum from all the recent discoveries from gravity waves, etc.
156	Yes	It seems like as a community we can make plans under various reasonable assumptions about JWST's success. If the actual performance of JWST is outside the range of these assumptions, then likely JWST is some kind of terrible failure. In this case, planning can be revisited.		The decadal survey science prioritization seems like it should not focus on the outcome of any one specific mission or facility. Keeping us on the current schedule would allow for clarity on a range of other important facilities and science areas.
157	Yes	There are many cutting edge scientific activities that small missions (like TESS/Kepler) are making that can be followed on by nimble missions and activities that are not "flagship" in structure to advance the field.		Loss of US leadership and the complete abandonment of any attempt to have a balanced portfolio of activity. In addition Congressional response may be negative and given the trajectory of JWST activities no guarantee that any schedule can be kept.
158	Yes	Goals conceived in 2019-2020 will not change if JWST begins successful operation in late 2020/early 2021. Digesting JWST results and their impact on future goals will take until 2024-2025. And a similar timescale would be needed to determine next steps if, god forbid, there is a crippling failure of JWST.		I see no advantage to delay. No single project, even a top priority one, should drive the Decadal Survey schedule. The DS process is important to projects across many wavelength ranges and subdisciplines. Requiring the community to adopt a holding pattern for 2 years with regard to prioritizing many projects just because one project has slipped behind schedule does not make sense. Such a step would jeopardize U.S. participation and leadership in current and future projects.
159	Probably not	Because it is necessary to know to what extent budgetary resources will be available for the kind of sciences we plan to pursue and prioritize them.		It is wise to know the most likely budgetary weather to plan a more realistic mission assessment.
160	Probably	"The show must go on". There are other priorities and JWST was a few decadal ago. While there are some uncertainties about budget and how the pending missions (WFIRST, etc) will be affected, the rest of the world and country will benefit from the next (on-time/regularly scheduled) Decadal survey, which should be conducted as if JWST were completed as scheduled. These recommendations and discussion are important for students and future scientists to set the course for the future of astronomy in the decades to come and postponing would be a mistake and disadvantage for the future generations.		The DS can still identify the big open questions (since the last DS) with an eye of the future. Therefore delaying sets a dangerous precedent, loses the momentum and creates confusion and lack of focus for on-boarding future astronomers. Meanwhile, the current JWST delay is a setback that creates some uncertainties, but delaying the DS for a few years seems to propagate delays forward to future generations, which is a danger and mistake, in my opinion.
161	Probably	Unless JWST is delayed by >3 years, I don't think the impact will be adverse.		Maintaining the standard timeline seems the best way to ensure continued NASA astrophysics strengths.
162	Probably	I understand it is difficult, but it's important to continue on pace with the decadal.		The Decadal survey is needed to prioritize many ground based facilities, and a significant delay could cause them problems.
163	Probably	It can be done. But what we get back will not be ideal - far from it. We need to focus on prioritizing science cases and portfolios of science (not specific mission architectures) the less we know about JWST.		The main answer to doing it on-schedule is not knowing what JWST will do, and uncertainty on budgets for flagship missions forcing the community into a "too-conservative" position that under-appreciates the real scientific value that we obtain from these large observatories.
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165	No	Everything depends on JWST performance		Community would be in a far stronger position if JWST successful; if JWST is not successful there would be rethinking about the future and project sizes. Also, WFIRST is evolving into a defacto mega-mission so the next decadal process will concern the 2030s.
166	Yes	Most of the space-based initiatives under consideration do not rely on JWST as a precursor.		Loss of momentum if the survey is delayed. Potentially, no big space missions ripe for implementation when WFIRST launches.
167	Yes			
168	Probably	The science priorities are independent of JWST operational status, it will be several years before JWST results impact scientific priority's. Furthermore, the technical, programmatic, and budget lessons learned from JWST are already largely learned. That said, a failure or cancelation of JWST could result in a significant change in the astrophysics budget, which would have significant impacts on the planning process.		Delaying the decadal survey sets a bad precedent and suggests we are unable to set and manage our priorities. The decadal is supposed to be a long term planning exercise and JWST was recommended 20 years ago. If we are truly thinking on that time scale JWST's status should not be relevant until the 2030 decadal survey.
169	Probably	It would be possible determine the next priorities under the assumption that JWST will eventually fly, and will bring its capabilities to the table. The next science goal priorities could be independent of JWST, although if it continues to delay and overrun, I could see how it may be a problem to the decision makers.		Teams (and funding for them) have all been geared up to deliver on the current schedule. Loss of momentum and funding for the teams would likely occur. Also, keeping all four flagship mission teams going, as well as the competing technologies within the teams would be very inefficient.
170	Yes	It is possible to prioritize, but knowing the state of JWST and WFIRST prior to that prioritization provides more information and insight on how to plan for a balanced program in this age of multi-wavelength astronomy.		<p>Delaying the Decadal Survey could provide more time to developing key technologies for Flagship missions (given that NASA resources were put towards this) - diminishing risk. It is not about the time delay. It is about the 2-yr time delay combined with the projected funding for the next Flagship mission. Given a delay of 2 years, increased funding for developing technologies could be provided, then post-selection, the Decadal cadence could be regained (shorter time to flight), given increased funding and the now-increased TRL for the key technologies (post-JWST launch). I see quite a bit of potential for what those 2 years could be used for, given some (not nominal) degree of funding. I also wonder if this would provide an opportunity to explore a Great Observatories option (or something like it) for the coming decades...given the state of readiness of each of the missions.</p> <p>It is not unusual for the Decadal committee to prioritize given programmatic uncertainty. The science of the 4 flagship mission concepts is robust and far-reaching (and these concepts, in some form or another, have been studied for many years now). The Probe-class missions offer an alternative (and potentially could help with TRL advancement for Flagship missions - starshade, optics, etc...) - and open the door for everything in-between flagship and probe-class. Multiple MIDEK missions would also have significant value (increased Explorer cadence). However, it is possible that not delaying the Decadal for 2 years might encourage a prioritization of lower-cost missions (Probe and Explorer-class)...ultimately delaying the flagship missions for much longer than 2 years...</p> <p>One possible path - Rather than NASA just asking for a delay, I would like to see a comprehensive plan regarding what would be done in the 2 years requested, and the desired NASA plan for beyond (for Explorer, Probe, and Flagship missions). I would like for the Decadal to meet as planned and review this plan (and NSF's plan if they have one), make a formal request on whether or not to proceed or make adjustments, and then meet again in 2-3 years for a more formal prioritization based on the outcome of JWST....just a thought.</p>
171	Probably not			It might become largely irrelevant on a short timescale, thus not serving its primary purpose. There might also be loss of momentum for 2010 Decadal Survey priorities.
172	Yes	JWST will not be the only major astrophysical facility in operation. It will do great science, but it is still limited to the infrared, and there is ground-based work and space-based work at other wavelengths which need to move forward regardless of JWST's operational status.		The danger is that our very powerful means of communicating our scientific priorities, the decadal document of 2010, will risk obsolescence. The landscape of facilities and missions changes, and science progresses in every one of NASA's science themes. Delaying the decadal would cause the field of astrophysics to lose its place in line to provide our priorities to Congress and to the science agencies, and my understanding a tweak of +0.5-1 year is not possible, even if extra resources were magically added to the Academy. (Extra resources don't have the experience and background, we could be risking the quality of the decadal survey.) JWST science is not the only science we do, not even in NASA. Forgoing science input now, in this environment, is not a good idea for the field as a whole.
173	Yes	Because we won't know the scientific implications of JWST until way after launch/commissioning/early science. Waiting that long will be too damaging for all the other projects/facilities/activities that need Decadal input. Waiting just for launch is not going to be informative enough to change any plans from a scientific point of view.		In my opinion the main dangers are 1) ID of new science questions or technology challenges that require immediate attention/prioritization to avoid losing US leadership; 2) impact on already planned activities,

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174	Yes	The delay is only a change with respect to the previous plan. Planning can take place based on a reasonable expectation of the JWST delay.		JWST is only one part of the overall activities, albeit a major one for Astrophysics. There are other science fields with activities dependent on NASA missions that will continue planning on the present schedule. A delay with the Astrophysics survey will cause the resourcing level to fall back and be overtaken by the other fields. At this time, with JWST facing delays, emphasizing the importance of the Astrophysics programs should be our top priority so that resourcing levels are maintained despite the JWST delay.
175	Don't know	I really hope that this decision will be undertaken with the concerns of junior researchers who are just getting started at the front of everyone's minds. I'm worried that there will be lots of squabbling over priorities, especially if there is no guidance and collective discussion, such as the one that occurs as we debate what should go in the decadal survey. As someone who is about to begin a junior faculty position and who is attached to a proposed probe (STROBE-X), I think careful decisions must be made that not only advantage people who are attached to projects that are well under way, like JWST, but also people who do other science. I am so very excited for JWST's data to come home and change the way we see the universe, but deep optical-IR isn't the only way to see the universe. Will room be made for those of us who aren't on JWST or WFIRST to join if those are the only games in town? Or is there going to be a cull? Will the community make sure that this does not disproportionately negatively impact white women and people of color, who are pushing to make real gains at integrating the field?		I think it's important for *thinking* about science to not be put on hold for JWST. It may be that JWST will impact the outcome of the next decadal survey, but we as a community should be planning together for what we think the 2020s will look like, including with the shift in JWST's timeline. Let's have the conversation!
176	No	Results from JWST are crucial inputs to the next decadal survey. For example, JWST will tell us whether it is possible for rocky M dwarf planets to possess atmospheres despite their host stars' high activity levels. This is critical information for determining which types of exoplanets we should prioritize observations of to characterize potentially habitable exoplanets.		Better to take the time to do things right than to rush and make mistakes. The landscape has changed since the Decadal was originally scheduled and proceeding with the original plan does not acknowledge the reality of this situation.
177	Yes	A decadal plan is always subject to later changes, but needs to be done in a timely way to take into account the full range of possibilities.		See above
178	Yes			
179	Yes			
180	Yes	Space science goals are independent of JWST. What can be done will be affected, for sure, but that shouldn't change our ability to decide what we think is important		We can't just sit around and wait for 2 years to decide where we are going. There is new science that needs attention now. In the absence of a unified voice, things will start finding piecemeal and what gets done will end up being set by private donors instead of the scientific consensus.
181	Probably not	We would have no sense for the data quality coming out of Webb to make an educated assessment as to what we would need next to address key science questions		
182	Yes	Astronomy has a well developed tradition of undertaking "decadal" reviews. Yes, we have had delays and cost over runs. But decadal reviews are essential for planning. Postponing the review just to suit JWST delay will result in loss of momentum for the -entire- astronomical community. I am sorry to say but a request by NASA to delay would be a clear sign of weak leadership at NASA.		
183	Probably not	Potential for additional cost growth that could impact future missions		Negligible downsides
184	Yes	It is more important to update the last decadal in a timely manner in the context of uncertain JWST operational status than to tie that update to the JWST commissioning schedule, which continues to slip.		Astronomy has moved a lot in 10 years and this needs to be reflected in national science priorities ASAP. The last decadal was "broken" anyway with the "divestments" from the budget crisis and a new set of priorities needs to be articulated.
185	Yes	The science is still basically the same. The only thing that is different is the facilities that we have available to address the science. See discussion below.		It is absolutely imperative that we NEVER set the precedent of delaying a decadal survey due to NASA programmatic problems. We didn't do this when HST was found to be defective after launch. Delaying the decadal survey will simply provide an excuse for more such delays in the future. Instead, we should proceed ahead with the decadal survey in which we clearly articulate possible different pathways in the event of: 1) successful launch and operations of JWST in 2020, 2) further delay of JWST past 2020, and 3) launch or deployment failure of JWST.
186	No	Results from JWST may surprise us! E.g. presence or absence of a nearby Earth-clone.		Advantages of delay include: Clarification of WFIRST status; better understanding of whether or not a nearby Earth-clone exists.

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187	Probably not	The next generation of both space and ground-based science priorities depends critically on the presumption that both JWST and WFIRST will achieve their primary mission objectives. If these two presumptions cannot be known, the priorities for what science we do in the 2030 era will naturally be compromised. We will not be able to check off our knowledge base of the early evolution of galaxies and structure in the universe, for example. And ground-based ELTs will not be an adequate backup for not having JWST. Furthermore, many of the interesting next gen space-based observational facilities in the UVOIR wavelength regime are building on the demonstration of deployable optical systems. If we don't have a viable demonstration of this in the form of JWST, it will be much riskier to assume such technology should be used in future designs. A delay to allow the Decadal to know the operational status of JWST makes a great deal of sense to me. I don't see us losing US leadership in astronomy by waiting for this. I do see a risk of us prioritizing science that may not be optimal in a world where we do not have a facility like JWST functioning.		
188	Yes	JWST's operational status is uncertain and unreliable, and given its track record over the last decade, it's wrong-headed to "wait" for any progress by JWST to prioritize other science goals.		
189	Probably not	JWST is a key mission for the future of astrophysics, and its disposition should be known prior to making decisions on moving beyond into the next generation of space physics assets.		The advantage to delaying is that current landscape of astrophysics will be better known. Another advantage to delaying is that more time is available to flesh on the concepts for the next generation of astrophysics flagships. The field of exoplanets is evolving rapidly even with current technology. There is no sense rushing for an arbitrary reason. US leadership is best served when we are pursuing state-of-the-art missions, missions that ONLY the US and NASA can do. We want to ensure that the next Astrophysics Decadal identifies such a mission, even if we need to defer the decision for a few years.
190	Probably			The question we exoplanet folks hoped that JWST would answer in time for the 2020 decadal was whether terrestrial planets in the habitable zones of Sun-like stars actually have atmospheres. If they do, then it behooves us to keep beating away at M-Earths to establish their habitability and search them for signs of life. If they don't have atmospheres, then M-Earths will have been an astrobiological dead end (albeit an astrophysically fascinating one). The habitability of M-Earths is therefore *the* biggest question for exoplanet astrobiology, and the decadal survey will occur before we have a satisfactory answer. My gut feeling, nonetheless, is that we should keep the timing of the decadal as is in order to avoid disrupting the entire astronomical community and because I suspect that even with a two year delay the answer will still be murky.
191	Yes	Astronomy and Astrophysics is a diverse and flourishing field in the USA. There are important science questions emerging, new technologies becoming available. A Decadal Survey can be conducted on schedule that will meaningfully prioritize science goals for the next decade based not only upon JWST and IR observations but addressing all of astronomy and astrophysics,		To delay the Decadal Survey sets a dangerous precedent that ALL of astronomy and astrophysics in the USA is reliant upon JWST and that the science questions it addresses are the only ones that matter. Astronomy & astrophysics continue to flourish in the USA and groundbreaking discoveries are being made at the present time (before the launch of JWST). It is vital to conduct a decadal survey on schedule to prioritize science goals so that the Decadal Survey can serve the entire field and that multiwavelength missions including JWST can be planned and their programs optimized to recognize the most compelling science questions. To delay the Decadal Survey risks allowing areas in which the US leads to lag behind the rest of the world as planning for the future (new missions, prioritization of science goals) waits for a delayed Decadal Survey. Science will be forced to continue over a period of uncertainty, unsure if groundbreaking new missions and technology that are already being discussed have any chance of going ahead without the Decadal Survey happening on schedule. It is important to retain the foresight and ambition to realize high-reward science programs for the next decade.
192	Probably	The full impact of JWST on astrophysics will probably become clear a few years after launch. Delaying the decadal survey to a time when the results from JWST will still be in flux doesn't seem optimal.		The identification of new science questions, though that argument is true for any delay for any reason.

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193	Yes	We shouldn't delay the rest of Astrophysics because of the delays associated with JWST. There is far more to Astrophysics than the science the will come from JWST. Also, the odds of a JWST failure to reach orbit or specifications are low. In the event of a launch or observatory failure, the community could gather for another decadal survey earlier rather than late.		Advantage: NASA and the NSF have delayed actions while waiting for the community to write the next decadal survey. I'm not too fond of this stalling tactic. We need to make a case for increased funding overall in the NASA Astrophysics budget in particular. While the NASA Astrophysics budget including JWST has remained flat in real dollars for ~15 years, the overall budget for NASA has kept pace with inflation. The relatively lower buying power of NASA Astrophysics needs to be addressed sooner rather than later.
194	Probably not	JWST represents one of the largest investments made by the US/European community. It is a powerful tool, one that we astronomers must be able to exploit before we can determine fully what the next set of investments we as a community will make in pursuit of science questions we don't yet know to ask.		How can we know what questions to ask without knowing what our next observatory (JWST) will be able to uncover? More importantly, what sorts of parameter spaces of OIR astronomy can JWST *not* shed light on that might be compelling to the community? On the other hand, if the the 2020 Decadal Survey *is* pushed through on schedule, will the 2020 DS simply ignore any OIR space observatory possibilities to avoid any bias or overlap with JWST?? That will be punishing if so because space observatories already take multiple decades to go from conception to launch. Delaying the next Great Observatory by a decade would remove any possibility of US astronomy remaining dominant.
195	Probably not	JWST consumes such a large fraction of astrophysics research funding that the uncertainties in terms of what's possible and what's not will depend strongly on JWST's operational status.		Advantages: JWST is drives more than just its own JWST science. Its science reach also has an impact on other subfields (e.g., radio 21cm surveys). Disadvantages: Some communities would benefit from some immediate clarity regarding the future of some facilities (e.g., GBT and Arecibo for pulsars).
196	Yes	While science priorities are matched to the capabilities of future instrumentation and missions, they should be mostly valid even if the instrumentation isn't able to answer them. Sure, priorities will be modified if JWST is not successful. But the Survey can be written in a way that doesn't invalidate the recommendations. The community is very adaptable, and can adapt quickly if required (especially with a well-crafted Survey document as the starting point).		The Decadal Survey involves more than just NASA. Delaying would have major impacts on NSF in particular, as NSF continues to have major challenges in developing new capabilities while supporting science, and operating existing facilities. Survey guidance to NSF should not be delayed. In addition, if JWST failed completely, a survey that replans how to accomplish those goals would take a additional couple of years minimum. Not really possible to modify the Survey without understanding what a replacement mission (or missions) would look like. That process is political and financial as well as scientific. Would not happen fast.
197	Yes	In my view this question only makes sense if there are major concerns that the JWST mission will not be a full success. However given the past investment by the community the exception should be that it is. Also, at what point would the authors feel that the operational status is known? Immediately after launch, after commissioning of the instruments, after successful science is done? As the timeline to launch is currently unknown, the entire survey could slip by many years (and slip again and again, much beyond the currently envisioned 2 years), with serious and unforeseeable consequences for all fields in astronomy.		The Decadal Survey is obviously not only focused on space science. Many other new initiatives critically rely on the outcome of the Decadal Survey.
198	Yes	Astrophysics has put all its eggs in one basket - JWST. As it stands now, success or failure of this mission will drive the future of the field. This is a major flaw in the overall planning, and the community needs to consider a more balanced mission portfolio.		The community needs to seriously consider the risk of making the entire science plan dependent on single points of failure. JWST may not work. What will happen to astrophysics in this situation? The community needs to face this reality and revise the approach to decadal science planning.
199	No			
200	Yes	I appreciate the danger of working from incomplete information, but we are better off planning earlier and more expansively than waiting for everything to shake out before starting. If another delay occurs, would there then be a second delay? At the cost of somewhat more work, I suspect the Decadal should evaluate science priorities in the context of a launch date the IRB report suggests is accurate, and add a section apiece on the delta priorities if 1) JWST is delayed again beyond the IRB date and 2) JWST cancelled. (Probably not worth anyone's time to evaluate the JWST-early case.) It might also be worth being explicit on whether JWST is still supported by the astrophysics community given the opportunity costs still inherent, particularly if it busts the cost cap again.		Some effect on WFIRST, as the starshade compatibility option being held by the project assumes a yes/no decision comes out of the Decadal. Delaying the Decadal Survey by 2 years, with no further action, would require the project to hold that compatibility well into Phase D at additional cost (and mass and power) for a cost-constrained mission. Presumably if it were prioritized by a 2020 Decadal and funded as an actual project, funds and personnel to support the compatibility hardware build, integration, and test would be made available from a newly-formed Starshade project. If it were panned by a 2020 Decadal, the starshade components would likely be scrubbed and free up the associated resources. The ambiguity will be the source of increased cost.
201	Yes	While JWST is expected to make interesting exoplanet science contributions, isn't providing a key piece of information that is essential inform the fundamental design of future missions the way Kepler did. The exoplanet community already knows what we want to do with future missions. The big question is whether we can afford to characterize Earth-size planets in or near the habitable zone of Sun-like stars that in the next large mission, or whether it's out of budget and will take two generations of missions.		1. More time to figure out how well the upcoming generation of higher-resolution and more stable spectrographs can find planets hiding among stellar activating. If it were practical to have an actual target list pre-launch from RV surveys, that would favor starshade designs. But if a future mission has to find its own planets, then depending on a Starshade becomes very risky. 2. More accuracy about when funding wedges will open up will allow for better planning. 3. Closer coordination with the Planetary Science Decadal Survey possible by waiting. (Conversely, NAS might not like the work being crunched together.) 4. Greater clarity about political landscape and implications for NASA's future.

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202	Probably not	JWST operational status will significantly influence perspectives on all facets of major space astrophysics initiatives. These include assessments of technical feasibility, engineering approaches, project management, and reliability of cost estimates among other critical factors that are relevant to a credible DS.		The advantages of a delay include gaining critical insights from JWST operational status and gaining time to re-establish the WFIRST project as firmly "in the pipeline." Otherwise, WFIRST may face re-prioritization in the DS that could lead to its elimination—or its reaffirmation—but at the expense of another new initiative of the highest merit. The disadvantage is the loss of momentum for current initiatives (LUVOIR, HabEX, IR Surveyor, X-ray Surveyor).
203	Probably not	JWST is expected to have a huge impact on many fields of astrophysics, albeit not all. For those fields impacted, it will be impossible to assess what is the most important "next step" without knowing whether JWST is working, and how well.		I think that a two year slip can be tolerated as I don't think that any immediate action will follow the DS. A possible interim survey that focuses on areas not impacted by JWST and considers some new avenues (e.g. PROBE missions) might be considered.
204	Probably	JWST is not the only telescope capable of doing cutting edge science. A decadal survey outside the scope of JWST heavy budget could be an opportunity for our community to start thinking about small, focused and fast (hence low cost) space-based and ground-based telescopes		The Decadal survey should be independent from the context of JWST. Our community has a lot of interesting challenges and ideas that should be discussed now.
205	Probably	Webb expected to usher in a new decade of exoplanet exploration whose hallmark is the study of planetary atmospheres. If Webb doesn't fly or perform as expected, ESA's Ariel mission will be all the more relevant. Perhaps we'd augment Ariel with another Explorer or Probe-class mission. However, I don't think the exoplanet community would change the longer-term roadmap significantly.		We might be able to define priorities, but I don't see Congress taking them very seriously when we haven't yet reached milestones identified in the 2010 DS. If the scope of projects undertaken by NASA typically require > 10 years to complete, then I'd say that a 10 year cadence for the DS is too short. We're already experiencing push-back on WFIRST, exacerbated by JWST delays. I suspect that even our own perspectives will be biased by the current delays. Who will be willing to suggest bold new initiatives when even Webb isn't yet up and running? I think plowing forward with a DS right now will give us a decade of business as usual. I support the idea of delaying the DS a couple years.
206	Yes	If JW fails, many mission concepts become viable that would not have been viable in the presence of an operating JW. A dedicated exoplanet eclipse telescope is one of them. So, do two prioritizations, one with JW and one without. If JW works but is debilitated, NASA can titrate between the two lists as appropriate.		There are lots of positive and negative ideas. One thing is certain: the 2010 advice is stale, and will only get more so. Just do two lists, one with JW and one without. Yes, it is more work, but it is not double. I am much more concerned that launch, communications, and computing capabilities will be different 2 years later than that we have JW or don't. JW is just one telescope, one of many data streams.
207	Yes	Neither WFIRST nor any of the four study missions depend on JWST. As a community we need to assume that JWST will work just fine and look beyond it, and there is no reason this is contingent on its launch.		We undermine the decadal process if we let its timing be negotiated every time it is done and delaying would set the precedent for that happening. None of the listed reasons make any sense for delaying - new science questions hopefully will always come up, we already know about launch vehicles and a surprise is impossible there, etc., and the impacts on future decadal, plus potential loss of momentum this time, completely outweigh any advantage for delaying.
208	Yes	It has been done before		I think delaying the decadal will do far more damage to the influence of the decadal and reduce astronomers' voices in the long term with the many places and entities that refer to the Decadal Survey for years to come
209	Probably not	Progress in all areas of Astrophysics will be effected by a successful JWST mission. Decisions on activities that complement JWST depend on the observatories status.		Advantages: Community will be more knowledgeable in terms of science questions, technology capabilities, and results from JWST, Euclid, TESS, SOFIA and other missions as well and ground-based observations and theoretical studies. Disadvantages: Ongoing studies and other activities being conducted in preparation for 2020 survey will have to be stretched out and/or extended at additional cost. Other institutions and countries may go forward making decisions on their own programs without US involvement.
210	Yes	NASA should actively seek advice at the regular time of the survey, even if the status of JWST is not known. At least a few options do not depend on JWST, and community members should scope out a variety of options (JWST works, JWST fails, JWST is greatly delayed with big cost overruns...) And besides, the decadal survey is important to other parts of the community, especially those people and facilities supported by the National Science Foundation.		The question here provides plenty of good reasons to hold the review on schedule.
211	Yes	We should plan on JWST being successful. It does not make sense to plan for anything else.		loss of momentum for 2010 priorities, opportunity costs
212	Yes	Neither the sub-field of exoplanets, nor any other astrophysics, depends critically on just one telescope, JWST. Both exoplanets and dark energy are not dependent on JWST (nor HST). Astrophysics has, and will, flourish with or without JWST. Put another way, if GMT or TMT were delayed, we also shouldn't stop the NAS Decadal. Indeed, delays are part of the Decadal assessment, as in planetary science. Finally, bloated budgets and delays of missions should not become a rationale for Decadal surveys being delayed.		

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213	Probably not	JWST ended up being way over budget and the recent delay is causing additional budget pressure to the 2010 #1 space-based astrophysics mission WFIRST. The political climate is further complicated because NASA does not have a head administrator approved by Congress and it is unclear what aspects of NASA's mission will get support from congress or the administration. I realize the decadal is an independent entity and run by the NSF, but I think there'd be more value in delaying the start of the decadal, at least until the next Congress is sworn into Office in 2019. Once Webb has flown and WFIRST has settled into a rhythm, I think it would be appropriate for the decadal process to begin.		The dangers in delaying the decadal are that it sets a dangerous precedence and could cause the USA to lose its position at being at the forefront of space exploration, be it from the ground or through space based observatories and missions. The advantages of a delay is that the process could occur under what would hopefully be a more certain scientific and political climate. My personal preference would be to maintain the current schedule, with the decadal making their best recommendation given the current climate, and have a definite plan for a mid-decadal in 2025 for any course corrections that might be required. A mid-decadal was talked about in 2015, but never occurred with any rigor. I think this was a lost opportunity.
214	Yes	The Gaia, Kepler, LISA Pathfinder, NuSTAR, and of course, HST missions have all delivered results to build on. TESS, SOFIA, and ground observations should still be expected to do all that is possible to inform an operational JWST. WFIRST is a less complex mission than JWST that should not be hindered by JWST delays and should be enabled to move ahead.		Conceptual missions including OST and LUVOIR that aim to leverage JWST segmented mirror technology and infrastructure will likely not fare as well if the DS is carried out prior to JWST's operational status being known. If the DS is not delayed, strong cases should be presented that high energy and gravitational wave missions are ready to lead and can be prioritized. Carrying out the DS in 2020 could invigorate early and mid career scientists and engineers to devote more time to demonstrating and achieving flight heritage for enabling deployment/assembly, propulsion, and instrument technologies needed for future large UV, optical, and infrared missions.
215	Yes	There is much more to astronomy than a single big NASA mission. There are many more science cases for NASA missions than those addressed by JWST. The Decadal Survey is a robust process with smart people involved who will be able to consider all factors.		Delaying looks like a panic response, giving the impression that we have no idea what we're doing. The other option is to acknowledge that daring to do big, hard, never-before-done things involves taking risks, including schedule and budget risks. Delaying the Decadal Survey because of the woes of a single mission, albeit a large one, seems like an overreaction. Stay the course.
216	Probably not	Anticipated discoveries will likely change the direction of many people's research in almost all fields.		A survey could be done on schedule, but likely would need to have a follow up in 2023 or so to update the priorities based on JWST discoveries and results. This is a substantial extra workload on any survey team, or would require creating a second team for the update, which decreases the efficiency of our entire enterprise. Many people would then have to go through the whole process essentially twice. The burden on the community is worrisome.
217	Yes			The decadal survey has a rich and laudable history, and has shown itself to be critical to the health of our field. It is a dangerous precedent to set to hold the decadal hostage to one facility's timeline.
218	Probably not			I don't see a loss in delay on the NASA side. Even on the NSF side, the status of JWST affects the field as a whole. It's hard to make informed decisions. But it should be done as soon as possible after JWST is operational.
219	Yes	JWST is an absolutely monumental mission, but it is not the entire field. Every year or so a big mission/new capability comes online (e.g. LIGO/Virgo, LSST) and JWST is not so much more important than any other that we absolutely need its information for the Decadal. For example, one of the current hot topics is multimessenger astronomy, and JWST is not particularly useful for this.		We risk losing US leadership. Look at all the mission coming out of China, mostly stolen concepts. If we delay they will just copy the probe designs and launch their own missions. Every year we will always know more and be better informed, so delaying is not particularly helpful.
220	Yes			
221	Yes	The goals for the next decade are diverse and do not rely on a single instrument (or should not). Any space instrument may fail at any moment; it behooves science to have many instruments pursuing many themes.		Science is under political attack, postponing a decadal survey only suggests weakness of the existing plans. There may indeed be further delays. The pace of science doesn't slow because of instrument failures or delays.
222	Yes	Prioritization will be difficult, but I'm not convinced that waiting will improve the situation. Also, NASA is only a part of the decadal and delay would harm other areas considerably.		
223	Yes	Because JWST and JWST-related science are not the only things in the NASA Astrophysics portfolio.		It is disadvantageous to delay prioritization for the next decade because it is lost time. JWST has been dominating the portfolio since 2000--almost 20 years!--and little has been realized from the 2010 decadal. Instead of postponing the decadal, and expanding the time that US astrophysics is marching in place, the process should take a more realistic approach to what can be achieved in the next ten years, and make actionable recommendations.
224	Don't know			Loss of momentum is dangerous to accomplishing the decadal survey
225	Yes	The science priorities for the next decade should be set based on an assumed successful JWST mission. It has already taken away ability to perform other science within astrophysics, so if heaven forbid it fails, other science that has been waiting in the wings for the last decade should still get priority over another \$10B attempt at the same science for another decade.		Ask yourself why is the decadal done on a 10 year time frame to begin with? It is the right cadence that allows for changes and progress in science, instrument, spacecraft, and launch vehicle capability to be infused in science prioritization and planning. Missions such as LISA, WFIRST, and starshade, for example, need more immediate feedback from the science community on their prioritization, and, if feasible within the given programmatic constraints, a new probe line for \$1B class missions should be established and setup earlier in the next decade rather than later. None of that prioritization work needs to wait for JWST, and without it, a starshade mission would slip too far in schedule to be part of WFIRST's prime mission (unless the priority for WFIRST is lowered), and the US will not be able to commit an exact

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226	Yes	Space science priorities should not be contingent on a single mission. First, there are a huge range of wavelengths and science that JWST does not represent. There is ample information to make priorities across a broad range of parameter space already - JWST is		There are a wide range of scientific fields that look to the Decadal Survey for guidance and also to reinforce the importance of particular goals. Delaying the Decadal Survey by two years for a single mission sends the message that the entire ground- and space-based astronomy and astrophysics scientific
227	Probably not			Early JWST observations will provide critical insight into which flagship mission makes the most sense for NASA to pursue next.
228	Probably			Delaying the DS could negatively affect NASA's participation in LISA
229	Yes	Yes. As major missions usually take a couple of decades, and the budgets for the 2020s are already assigned to JWST and WFIRST anyway, this next decadal report will probably realistically be for the 2030s anyway.		I see no dangers associated with any specific timescale for decadal reports. The main dangers are an over-reliance on decadal reports.
230	Yes	We can certainly prioritize science goals at any time. This prioritization will have to and always has taken into account the status of future telescopes and instruments. If the status of JWST is too uncertain to allow this, then there is no reason to believe that JWST will launch in two years. If, on the other hand, JWST is virtually guaranteed to launch in two years, the Decadal Survey can take it into account. So there is nothing to gain from delaying this. Space missions have always been late.		We can always find reasons to delay any survey with the argument we know more in the future. A delay now would be a political delay to time the next Decadal Survey when it is opportune for NASA. This is cherry picking.
231	No	It would be a complete waste of time to have to justify JWST and its science mission all over again in this new decadal.		Giving that JWST and WFIRST both hang in the balance in different ways, it would be a total waste of time for us to have to re-prioritize both these missions all over again. So delaying the decadal till JWST flies seems to be the sensible option. The argument has been made that Planetary Science wants their
232	Yes	Priorities can be provided with contingencies.		It's essential to maintain the current DS schedule so that other agencies, in particular, NSF can exploit critical timing with MREFC planning. NASA can write contingencies in the DS prioritizations. Moreover, keeping the schedule regular is important for maintaining credibility and community coordination. Is the
233	Yes	JWST is either operational or not. And given the latest reports of bolts coming off in vibration testing, why would the DS prioritize another large mission of this scale? So it is best to just go ahead and make plans anyway. Don't see that it will make much of a difference		loss of momentum, rest of the astrophysics program still needs to proceed with or without JWST
234	No	We have to wait for very early JWST science results to design future exoplanet science priorities.		
235	Yes	The scientific priorities are greater than even a great instrument. Obviously surprises are possible but most likely JWST will provide data which are consistent with our current vision of priorities in its initial year of operation and thus not upset our vision of scientific priorities.		It upsets the rhythm of Decadal Surveys and puts the rest of science in a holding pattern while the impact on important but limited scope of JWST of JWST's performance is assessed. The cost of flagship missions has a central influence on the budget but the broad spectrum of science that NASA pursues will suffer.
236	Probably			Delaying the DS is likely to lead to a bottleneck in funding for large ground based projects, and to extend the period of uncertainty for existing ground based facilities that whose long-term funding is unknown.
237	Yes	I understand that JWST could mean big things to the community, but I think this idea is a slippery slope. For example, if JWST gets delayed again, would we continue delaying the Survey? It seems as though, especially in the next decade or two, there will be a large		Another danger of delaying (other than those listed above) is that it takes some of the focus away from science in the White House, which I believe is *desperately* needed right now and through 2020.
238	Probably not			
239	Probably	The science isn't going to stop, regardless of the JWST status. Neither should planning/implementation.		
240	Yes	Science, and thus scientific priorities, move forward irrespective of one NASA mission.		Delaying it diminishes the importance of 2010 priorities and of the process itself.
241	Yes	There's lots of astrophysics under the sun.		JWST should not drive the Decadal, it must be the other way around. I understand the uncertainty causes problems, however there is no guarantee that we will know more about JWST's status even with a two-year delay. A delay is completely unfair to science that is not addressed by JWST.
242	Yes			The science and technology driven identification of top astronomical priorities seems independent of JWST. How those priorities are implemented can be decided later but I don't see any reason to cripple this highly efficacious mechanism that the community has developed.
243	Yes	While of course it would be nice to know JWST's status, but it's not actually clear to me that future astrophysics space science goals actually require this information. I feel that, if necessary, a Decadal Survey could outline multiple paths based on different outcomes for		I strongly disfavor delaying the DS. There may always be reasons why one *might* wish to delay a Survey/Review, but the rest of astronomy/astrophysics marches on and requires a regular cadence of prioritization. Disrupting that schedule would reduce the reliability and credibility of the process, and
244	Yes	The Decadal Survey should review all of astrophysics, including ground based initiatives that are not dependent on JWST. Planning for the next decade should take into account any uncertainties in JWST status, but should not wait indefinitely until they are resolved.		Delaying the Decadal Survey for a single mission (JWST) could seriously compromise ground-based astronomy and international collaborations.
245	No	JWST is sucking all the money out of Astrophysics within NASA		The current administration is not paying attention to the Decadal Survey anyway, so might as well wait until we get a new government.

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246	Yes	JWST's discoveries should NOT influence the 2020 decadal survey. It's outside influence on the current budget ought not to influence any future *science* decisions, goals or appraisals. JWST is not special and should not be treated as sacrosanct.		
247	Yes	While JWST has a number of goals, The entire non-JWST-reliant astrophysical community would still benefit now from a prioritization of goals. My sense is that the decadal review should move forward on the assumption that JWST will work (at some point in the next		Gravitational-wave science has been vitalized/revitalized with the recent LIGO detection, making GW astronomy and multi-messenger astrophysics a major target for the coming decadal survey. A several-year delay could jeopardize the acceptance of US contributions to LISA, and might be harmful for rallying
248	Don't know	The failure of JWST would be an awful blow to NASA's astronomy and astrophysics program. Depending on the failure more, NASA might want to redo JWST, thus making the Decadal Survey moot. So that's an argument for delaying the survey. On the other hand, if JWST is successful, as we all hope, then two years will have been lost on the Survey. I don't know how to weigh these two possibilities.		One advantage of a delay might be maturation of the studies currently underway for new large missions (e.g. Lynx, etc.). Another might be important advances in launch vehicle capabilities (to steal from your examples). I believe the main disadvantage is the extension of uncertainty in the direction of NASA's program. Honestly, I am not the best person to ask about management decisions.
249	Yes	Many of the other missions and projects proposed are complimentary but not entirely reliant on the success of JWST to be successful or worthwhile. For example, exoplanets are a major area of interest and while JWST is an extremely valuable tool for exoplanets, there are other opportunities that can continue and be planned without the exact status of JWST being known. In other fields such as studies of the CMB and the associated polarized galactic dust, galactic star formation, and molecular cloud surveys, the status of JWST isn't necessary to plan out the recommendations for the Decadal panel.		Right now the various sub-communities are planning Decadal recommendations, white papers, etc. and delaying them might provide more time for preparation, but will likely just result in either the plans going along as currently scheduled, or the activities being pushed back to coincide with the later date.
250	Probably not	Huge potential effects on NASA programs.		Clearly a benefit to NASA to delay. I worry somewhat about lack of guidance for NSF/DOE programs, but am not familiar with any details.
251	Yes	The decadal survey addresses all areas of astrophysics including space-based and ground-based and is tasked to evaluate and rank the compelling scientific opportunities of the time. JWST is an important part of the broader issues of infrastructure and organization that the committee is also tasked to survey but it should not be given such a high status as to delay the survey.		Again, JWST is only one of a large number of missions and concepts many of which rely on (real or anticipated/proposed) funding and development schedules for planning, purchasing, hiring, etc.
252	Yes	Absolutely yes. Historically, there have always been projects that are about to become on line at, or just after, the Decadal survey. The approach that has been taken is to make assumptions about those missions and proceed onward. Historically this approach appears to me to have worked extremely well and thus the community has proven that such an approach works.		This is a complex question to answer but, by and large, I see more advantage to continuing on the current schedule. I am strongly influenced by the historical fact that the highest priority space missions launch, on average, 20 years after the Decadal Survey that recommended it. Thus, there will be plenty of time to adjust if JWST results actually play a critical role in redefining the future. Moreover, and although subject to argument, I believe that despite the outstanding success of previous high priority space missions (HST, Chandra, Spitzer) that no mission definition paradigm shifts ever resulted from the early results of these missions on the next Decadal Survey, rather the detailed arguments for certain scientific goals were sharpened.
253	Probably not	Real planning is only possible after the constraints are known (i.e. funding profile)		The DS is a document which highly influential and respected. It should not be diluted by the uncertainties surrounding the JWST
254	Yes	JWST is not all of astronomy. There are many other fields that need recommendation and direction		Delaying sets a bad precedent. Science planning and prioritization should always proceed at a higher level than programmatic.
255	Yes	The launch of JWST is hugely important programmatically, but (on the assumption that it does not fail) its precise launch date does not alter the scientific priorities for the next decade across all wavebands.		The Survey must not be delayed. This would represent a dangerous break from precedent in a survey that the community has worked hard to place as the key priority setting exercise for astrophysics. The 2010 survey was pretty much hijacked by the delays and cost increases of JWST and we should not let it take another decade. It will not be possible to start new things (e.g. some of the interesting probe class missions being studied) without some kind of blessing from the Decadal Survey. Their job will be to look at the portfolio balance and try to adapt to the reality of a delayed JWST and a late starting, cost overrunning WFIRST, but there is no need to wait for JWST launch to do that. The science landscape has changed since 2010, with gravitational waves, neutrinos and other science topics having come of age during this decade. The time for a new Survey is now.
256	Probably not	The very concept of flagship missions is riding on the success of JWST. If NASA can launch a successful \$8-9B satellite that then returns incredible science not possible at a smaller level, the rationale for flagship missions is secure. If either of these (successful launch, incredible science) fails to emerge, I cannot see the US gov't continuing to support such efforts.		Currently we have a backlog of selected missions relative to available funding for both ground and space based large activities. Delaying at LEAST two years - even better would be five - would allow missions such as JWST, WFIRST & LSST to either demonstrate their scientific power (JWST, LSST) or get firmly into phase C/D (WFIRST). Only then do we need to distract the community with the question of what next; right now, we're asking the question not because it's the right time, but because that's what the calendar says.
257	Yes			People need to know the goals of that survey to plan their research.

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258	Probably	JWST delay will primarily affect prioritization of the large missions, but they could easily consider a couple of scenarios for the large mission case and allow the rest of the decadal survey work to proceed normally.		The decadal survey does much more than prioritize large missions. The medium scale projects are much more time critical (for both ground and space projects).
259	Yes	I think the issues with JWST speak strongly to the dangers of large flagship missions who cost investments make it such that they cannot easily be terminated. I do not think our goals will change. I think we may need to assess risk and cost more carefully and having JWST hanging over things will lead to a more honest conversation		I see no advantages. I see dangers in not evaluating how the science landscape has changed causing delays in new programmatic priorities. I also think that its important to note we do not want JWST or the flagships to drive the Astrophysics program. The program drives them. If they are delayed, we need to not put everything else in limbo or on hold.
260	Yes	Decisions need to be made to guide the next decade, and those will not really be impacted by the exact JWST launch date. I suppose if JWST were to fail (heaven forbid!) or to have significantly reduced functional capability, then the community would have to consider the priority of a JWST replacement mission relative to other projects. But even in that worst-case scenario, I think the new JWST launch is before the end of the decadal study, so a pivot (or a delay to the conclusion of the decadal) should be possible. I suppose that would add cost to the decadal, but that would be tiny compared to the replacement mission cost.		
261	No	Even assuming deployment and check-out of JWST go smoothly we would need to wait 2 years to know enough about the resulting science to use JWST results for guidance. E.g. Are the biggest breakthroughs at long wavelengths or short? The answer would change the weightings that the Decadal would give to OST and LUVOIR. The outlook for JWST lifetime factors in too. We would need to wait about the same length of time, 2 years, for the JWST team to investigate and implement propellant saving measures (which are already known in theory) that could, in principle, extend the lifetime of JWST by many years. A long-lived JWST would change the science landscape, and invoke significant run-out operations costs. So it may be narrowly "possible" to prioritize missions in 2020, but it would not be prudent. Waiting is wiser.		Even before this delay, the NET 2025 launch for WFIRST meant that the Decadal was happening too early, as no flagship could start construction until WFIRST launches, and so could not fly until ~2030. Now those dates have become 2027 and ~2032. That would argue for postponing the Decadal for at least 2 years, and perhaps even as much as 5 years, to get the planning period back in synch with the real funding opportunities. Having the Decadal happen for 2020 will not hasten the arrival of new flagships. In fact, decisions taken in 2020 could well come to seem ill-advised 2 - 5 years later. By the time those extra 2 - 5 years have passed the scientific and technological situations are likely to have changed greatly. The background science, not least due to JWST itself, is progressing rapidly. This is especially the case in the field of exoplanets and bio-signatures, TESS being a major example of a game changer. Moreover new launchers from SpaceX (F-H, BFR) and Blue Origin (New Glenn, New Armstrong) could alter both the physical constraints and the financial constraints on new astrophysics missions positively. It is even possible that Commercial Crew, with operational flights beginning ~2020, will reopen the possibilities of on-orbit servicing (as for HST), assembly, and construction in a cost-effective way. (I have written about this in Space Policy - 2016, vol.37. p.65, also on arXiv:1608.01004.) A few extra years will clarify just how much bigger and cheaper flagships could become. Similarly the uncertain situation with the ELTs will be clarified in a few years: will there be one, two or three? How well will their advanced AO systems work? In addition, the #1 ground-based initiative from NWNH in 2010, LSST, will have results starting in 2021. These too may well change the science emphasis of the next Decadal. For all these reasons delaying the next astrophysics Decadal by 2 - 5 years is advisable.
262	Yes	We do not know what we will learn but the topics we will learn about		I see no problem with a delay. The last DS put did focus on several large missions which were dismissed by us after a brief period (LISA), delayed by almost a decade (WFIRST vs. Euclide), or are on track but with potentially limited impact (LSST). Fortunately, our partners filled the gaps.
263	Probably			impact on already planned activities (stalling and maybe losing funding/personnel), loss of momentum in early efforts for new programs
264	Yes	It better be possible. JWST is one experiment, and its launch date has been delayed by 13 years. One may hope that this is the last delay but any claims that this is definitely the last delay seem misguided. Other important science should not be sacrificed or be delayed.		JWST should not hold an entire field hostage. Other interesting science should be planned and prioritized. Otherwise it will be unnecessarily delayed.
265	Probably	The operational status will affect many multiwavelength science objectives, creating a wider envelope of uncertainty than would be if the study was delayed. However, the Decadal Survey has successfully been carried out in the past even with similar uncertainties. The one main difference from the previous Decadal surveys is the dominant fraction of the JWST investment in the overall astrophysics space science portfolio.		There are several nascent midscale projects (ground based) which would benefit strongly towards realization with a decadal survey endorsement. Delaying the report would continue to place these projects in limbo until the decadal survey is complete. There will be an ongoing winnowing of technical expertise for these projects as the decision point recedes further into the future. Several of these projects are not strongly tied to JWST science. There would be some advantage, however, in knowing whether JWST is successful insofar as this will commit a substantial fraction of the budget for several decades, which may make some of the upcoming projects financially unfeasible.

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266	Yes			It would further delay the prioritization of astrophysics goals, including (large) missions, the implementation of key programmatic opportunities for others, ultimately impacting progress and the development of strategic milestones. It would impact momentum supporting Astro2010 priorities, the ID of new science and mission concepts, and further erode US leadership as a pioneer in astrophysics investigations. It seems possible for the Committee to prioritize goals in the absence of JWST, thereby mitigating long-term impact of its delayed launch.
267	No	JWST sucks much of the funding from SMD. We have to know if that has stopped before planning something new.		Goals of the recent DSs have not been completed in the time expected, so there doesn't seem to be a strong reason to stay on the current schedule. Implementation of the recommendations of the last DS were strongly affected by JWST. It will be pretty much impossible to make meaningful recommendations on the space component of the DS without knowing the status of JWST and WFIRST. This is strong argument for abandoning flagship missions in astrophysics and limiting ourselves to Explorers and a new Probe class capped at 1 or 2 billion dollars.
268	No	JWST has accrued so many resources and expectations that its degree of success will in fact write the agenda of what is necessary to focus on.		I don't see any real danger in a delay. WFIRST is also moving slowly, direct imaging of exo-planets from space looks quite challenging, being now a demonstration mission, where technology is still under development. Other mission concepts are quite in their beginning as to have realistic simulations. Also there's a need to have some more observations of exo-zodiacal light in other exo-planets systems. In addition, the Cosmic Microwave Background studies are much stuck due to uncertainties in how to deal with galactic foregrounds and effectively removing CMB lensing. 2 years is a reasonable time lapse to be more up-to-date in the main lines of the decadal survey, in order to evaluate how ground experiments and observations have advanced in those major topics. It will also give a better opportunity for other new concepts, say a mission with a Starshade to directly image other planets.
269	Yes	Most or all of space-based initiatives under consideration for the 2020 Decadal do not rely on JWST science as a precursor		Loss of momentum
270	Probably	It's a political decision. Who gets the most votes for whatever.		I think you should try to prove the little bang theory. Let's assume black holes are not matter but are pure energy compressed by gravity. Assume black holes blow up at some energy level other than all the energy in the universe (big bang theory) - maybe because of spin. Work backwards and figure out how long it would take all the energy in the universe to form one black hole. I think you would get a larger number than 13.8 billion years. (Given a big bang black hole - that would mean we are inside the big bang black hole event horizon.) I would hypothesize that a galaxy (like the milky way) is the approximate amount of energy needed to create a little bang. I haven't got any theory about how often these little bangs occur but maybe some of the supernovas are little bangs.
271	Probably not	Will not be able to know how much money and manpower can be allocated to other missions/projects		If we delay the DS by approximately 2 years, we would be sending the wrong message to anyone involved (from companies, to scientists, to PIs, to NASA and government approving the funding etc) that there is no penalty for holding hostages all these other missions and people working on them! We would excuse repeatedly delays and pitfalls in programming, managing and executing such a big project. On the other hand, as I mentioned above, if we proceed with the current schedule, then JWST's operational status will not be known at the moment and thus in turn we will not be able to know how much money and manpower can be allocated to other missions/projects. I think the compromise would be to push JWST teams to come forward with its true operational status much earlier than what is anticipated (and would be convenient just for them).
272	Probably not	The last several decadal surveys have foundered on what turned out to "unknown unknowns", the last of which was actually JWST cost overruns. This time, JWST issues are at least a "known unknown": It's known there will be issues, it is not known how big they will be. It seems foolish to make an attempt to prioritize our community's needs when it is clear we don't have the maneuvering space to do this properly.		All of the above, plus loss of credibility for the agency. Although everyone knows that the decadal report isn't a carved-in-stone list of must-do deliverables, there is at least some sense that the agencies will strive mightily to have these priorities a reality. On the current schedule, we run the risk that this won't be possible, and the report will be doomed as soon as it is delivered.
273	Yes	JWST isn't the only game in town.		I have a timely need to raise awareness of a non-JWST topic/mission: Prompt Optical Spectral Measurements of Gamma-Ray Bursts
274	Probably			
275	Probably			
276	Probably not	It is not just JWST, it is the turmoil and anti-science stance of the present administration that makes the funding situation uncertain.		I am more concerned about the erosion of US leadership in astronomy than a loss of momentum in the 2010 priorities. But the advantage of a delay until both JWST operations and the political situation in Washington are clearer and perhaps more congenial outweighs even that concern.
277	Yes			

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278	Yes	There is always a risk a prioritized mission may or may not work, but that cannot delay the community-driven process to determine what the scientific priorities are for the coming decade and the likely missions, facilities, programs and projects will address them...the fundamental point of the Decadal Survey is to prioritize the _science_ goals and outline the funding needed to accomplish those goals. If something catastrophic happens with JWST, then the Academy process allows for adjustment through the CAA.		Delaying the survey disrupts other agencies and institutions by not answering pressing questions and needs they have now and are not connected to JWST being operable or not...as well as disrupting prioritization of new missions at NASA of all sizes and scopes. Adjustments can always be made in cases of emergency and will be through community dialog and decision making (e.g. Academy studies related to Hubble Servicing, or WFIRST initial opportunity...
279	Yes	At worst, we can design two plans one with and one without an operational JWST.		I think it is absolutely critical that Decadal planning not be affected by the implementation of past Decadal plans. I think this sets an extremely bad pattern that gets away from the overall goals of Decadal plans. By sticking to our decadal strategy and standard timelines, we ensure that the entire community is always on the same page. In discussing followup missions within NASA and beyond, many teams have already placed significant effort preparing for the decadal call. I do not wish to lose that momentum. Moreover, this change will have a knock-on effect for other nations making their long-range plans. I feel as if the 2010-2020 has already become the decade that JWST ate. I do not wish to see that extend further. As an American working abroad, I think it is absolutely critical that the Decadal process continue on its normal schedule.
280	Yes	The astronomy community cannot keep waiting for JWST forever. Time to move forward. The only issue is the financial one. It should be possible to set priorities regardless of the JWST timescale at this point. I don't see why it is relevant; it is decided and the money is mostly spent or allocated. Other missions/priorities need to be discussed on schedule.		Loss of momentum, competition from projects in other nations—we need to move ahead on schedule so we can work well with our international partners.
281	Yes	I have no idea what you are doing here. What is the rationale for delay exactly? JWST may fail at launch or during deployment? If so, how is this evaluated? What metrics are used to justify delay of the DA? Something else? If so, what? Furthermore, why is this being circulated on a google form with default settings, with no supplemental information by COPAG? Why is it not being circulated by NAS? To whom is it being circulated? If not the whole community, then how could the results be evaluated fairly?		I have no idea what the impact of delay would be. Shouldn't you begin with a statement by NAS stating that it is possible? Why is their voice no heard here? Is this an end run around them? It is hard to imaging how one delays the coming of a decade? As a person who works inside the capital beltway, I feel the next decade (and decadal review) can't come soon enough.
282	Yes	The decadal process is a very important process for the US astronomical community. In planning for future scientific missions in space, the community generally makes the assumption that planned space missions will be successful following their launch. The highest space-based priority in the 1991 Decadal survey was the Advanced X-ray Astrophysics Facility (AXAF which was renamed Chandra after launch in 1999). The top priority the 2000 Decadal Survey is the James Webb Space Telescope, while the top priority in the 2010 Decadal Survey is WFIRST. The first of these missions is operational and the second two are in development. By the end of 2020, when a decadal process begun in 2019 would be completed, HST will have been in operation approximately 30 years, while Chandra will have been in operation about 21 years. Neither JWST or WFIRST will have been launched. Currently the Europeans are operating XMM-Newton, an X-ray mission launched in 1999, participating in HST, and have laid out a plan to launch Euclid (an optical and near-IR mission) in 2020, Athena (an X-ray mission) in 2028 and LISA (a gravitational wave observatory) in 2034. Since the development of a large space mission can span a decade or more, waiting to know if an approved, but not yet launched mission is successful can significantly delay the development of next generation space observatories. Since it can take many years to build a new major space observatory, significant delay in prioritizing new missions may mean that when a current space mission ends, there will be no access to new observations from a US mission.		Carrying out the Decadal Survey on the current schedule would help ensure that the US does not fall behind Europe in planning and carrying out future scientific space missions. The loss of research opportunities in the US may mean that many of our best space astrophysicists will choose to work outside the US, where there is more access to new space-based observatories.
283	Probably			We should not lose the momentum of a regularly scheduled strategic planning process.

	A	B	C	D
284	Yes	The goal of the survey is to indicate the state of astrophysics each decade and propose avenues. If the status of JWST is not known, that should be reflected in the survey and options for contingencies should be indicated. Delaying the survey would have the effect of pretending an importance to JWST that it should not have. If a catastrophic failure occurs, if JWST blows up on the launch pad, if it fails to deploy, or suffers from an HST-caliber problem, it is important that we have a roadmap forward. A delayed decadal survey would then be in response to such contingencies rather than providing a way forward if it continues with the normal timeframe.		The danger of delaying the survey, as eluded to above, is that it is hoping for an outcome that is not known. The decadal survey is not the JWST survey. We still need to know how to prioritize such things as our radio telescopes, our optical telescopes, our educational pipelines, our cosmic ray telescopes, our gravitational wave detectors, etc.
285	Probably	JWST's operational status is a binary question. It will either work as advertised or it will not. The consideration of either outcome should be straight forward. It is very unlikely that should JWST fail to operate as advertised that NASA will fund another one.		Delaying implies that the JWST is the focus of the next decade. While assuming it launches and operates as it should than it will contribute to the directions that the community will focus for the next decadal survey. It is the questions and science that the community wishes to pursue now that should be the focus of the decadal survey.
286	Yes	It was possible in 2010. Why would it not be possible now? The publicly available information suggests no more than a remaining speed bump. So why the desire for a delay? How about you are honest and say that this is yet another shameful attempt to protect that which was jammed down the astrophysics communities throat - WFIRST. The sooner we move on from that mess the better. It's a shame that it appears that congress is going to have to make the decision for us.		The scientific impact of missions in current development should not limit future strategic thinking. It's not like the decadal is some sort of sacred process.
287	Yes			It would set a VERY DANGEROUS precedent to delay the decadal survey. DO NOT DO THIS!!!
288	Probably not	JWST will provide orders of magnitude better throughput in the mid-infrared, which will likely lead to significant new discoveries!		Part of the main responsibility of the DS will be to decide what the successor to JWST will look like. This will be very difficult without knowing what JWST reveals.
289	Yes	I would argue that uncertainties due to JWST launch date and WFIRST HAVE to be factored into the decadal review, and not sidestepped.		There are important lessons to learn from the JWST hurdles as we move to more ambitious mission concepts. The decadal review can provide clarity and perspective on these uncertainties.
290	Yes	Our science goals are not really going to change. We can do a delta-report if the situation regarding JWST or WFIRST changes significantly.		I had just started my answer when your form decided I was done and recorded it!... I was saying: It would set a VERY DANGEROUS PRECEDENT to delay the decadal survey. DO NOT DO THIS!!! We know the science we want to do in the coming decade. We should proceed ahead proceed ahead on the assumption that JWST will actually launch soon after the current NET date, and that WFIRST will in fact be terminated. The survey should then address whether some version of WFIRST (or a successor concept) should be a priority in the next decade. If the situation changes significantly, (e.g. JWST is further delayed, or has significant technical issues after launch, or if WFIRST manages to survive) NASA can request the NRC to do a delta-report discussing the changes to the decadal survey that should be implemented, given the new environment. But we should proceed NOW, on the best assumptions that we have regarding the future. If those assumptions prove to be incorrect, there are already mechanisms in place to deal with that. Delaying the decadal survey simply gives NASA an excuse to do whatever it wants, without proper community input. WE CAN NOT ALLOW THAT TO HAPPEN!
291	Yes	There are more important missions to astrophysics then JWST		You're all stuck in the sunk cost fallacy. Let's move LISA forward!
292	Yes	While the astrophysics space science goals have been seriously delayed by the JWST development and launch delays, it seems that those goals on the assumption of no further delays can be evaluated fully now.		If the Decadal Survey is delayed, the time at which its recommendations can be followed will certainly be delayed, even if there are no further JWST development and launch delays. Also, such a delay would appear to give much more importance in the NASA program to what can be done with very large telescopes than the astrophysical science that can be done with much smaller NASA missions or with NASA joint participation with other space agencies in missions that are now being developed.
293	Yes			I strongly back carrying out the Decadal Survey on the current schedule. The future of space astronomy and astrophysics can not be held hostage by delays in a single mission, even one as prominent as JWST. I have full confidence that the community can chart an exciting path for the future NASA astrophysics program on a timely schedule. If anything, the decadal process should be accelerated as much as possible in order to get next steps going early in the next decade.
294	Yes			
295	Yes	It is a major mistake to delay the Decadal Survey until after JWST launch. The community knows the risks involved if JWST does not work and the potential impact on the community. Better to do the Decadal Survey building in contingencies and options. A delta survey one year after JWST launch would be a better approach fine tuning what we planned based upon the operational state of JWST and HST (which remains the critical component in space astronomy)		No advantage in delaying. Everything to gain by doing the Decadal Survey on schedule

	A	B	C	D
296	Probably not	we can't plan for the next decade without knowing whether the single biggest new discovery machine is working better or worse than planned. our planning needs an accurate starting point		2 years is a worthwhile delay to make sure that the entire 10-year plan is not badly undermined from the start
297	No	Too many unknown		having a decadal now would not be very helpful
298	Probably not	I think that the physics of the orbit at which it is to be positioned is poorly understood and that this is mission critical for LISA.		Loss of public interest
299	Probably	I think we should assume JWST success and deal with the outcomes as they come. I cannot imagine radically changing the selection of a future mission, that is likely 20+ years down the road on the basis of a recovery plan for JWST. WFIRST is more complex, but I think I would proceed. Further, I do not see the logic of delaying a decadal survey if the major recommendation of the last will be ignored. The decadal should assume JWST success and perhaps react to the lack of commitment to WFIRST, but also give input to the balance of the program and prioritize any flagship concepts ready to move forward.		In some sense, the NSF and NASA processes proceed faster than a decadal timescale in any case. Therefore, I do not think a delay is wise. The ground-based NSF portfolio needs input now and delay would have serious consequences for US leadership in key science areas. I also feel with ESA moving forward with ARIEL, and the M5 concept studies, it would be very healthy for NASA to have a clear priority for a future flagship sooner rather than later.
300	Yes	When major missions are at turning points is not a bad time, but a very good time for the community to weigh in.		Delaying needed planning exercises because of temporary circumstances is a dangerously slippery slope. It is too easy to find reasons to postpone such a study, and breaking our rule of studies every decade will lower the threshold for new excuses in the future. More than enough prioritization questions are on the table now to justify a community study without delay.
301	Yes	The decade analysis is a "snapshot" of the status of the field at the time of the report and the communities best estimate of "where to go" for the next 10 years. By the time JWST is operational, data recorded and analyzed it will be 2025. To late for a decade report. Clearly if, for some reason JWST launch results in a disaster, then the baseline conditions of the report all change. And that should be dealt with at that time. The astronomy community will lose credibility in Congress, the Executive Branch and the people of the USA. if the process is delayed. Our graduate students, postdocs and young professors will lose direction. With missions taking so long, it is they who will suffer. Lets us not get into the dead end of JWST being "too expensive to risk a launch"!		
302	No	Given the proposed (somewhat arbitrary) termination of WFIRST and the delays in JWST it is unclear what or if a coordinated plan exists at the level suggesting budgetary objectives.		The JWST have already caused a loss of moment in other areas of astrophysics. As such until JWST is launched – the idea of committing to new investments will only further cloud this process.
303	Probably not	JWST provides a testbed for the detection of small planets atmospheres and biosignatures. Its input will be necessary to optimize the design and capabilities of future missions.		One danger is on already planned activities which require or are based on the performance of JWST. We also need time to identify the next goals after the potential detection or not of biosignatures.
304	Yes	If JWST works, fantastic. If JWST fails, we're not building another one this decade. There's always room in Decadal Survey plans for changes, including new, exciting opportunities, and dealing with setbacks; should JWST fail, it would be reasonable to consider whether targeted, smaller missions might accomplish some of its goals. JWST has dominated US astrophysics planning for quite a long time already.		My serious concern is that the Decadal Survey may be necessary in order to garner the needed NSF support to fund the 30-m class telescopes, TMT and GMT. Without NSF support one or both projects may fail. This would probably also be a good time to re-assess the astronomy community's interest in WFIRST; if it remains high, a clear statement to that effect would probably help WFIRST get its funding back.
305	Probably	I think that the main impact would be on prioritizing the next Flagship mission, but almost everything else (Probes and science in general) could be prioritized. Another point is that the landscape changes very quickly (e.g., new understandings since gravitational waves have been detected), so there are always things we are waiting for.		I think the probes, in particular, will lose a lot of momentum. There have been very substantial efforts based on current technologies. Teams have been assembled and are working hard. Given uncertainties about JWST and WFIRST, I could imagine having a Decadal without Flagship missions.
306	Yes	There are several very large astrophysics projects beyond JWST (WFIRST, GMT, TMT, LSST, etc.) that will yield unique data and scientific discoveries, regardless of the status of JWST. It is important to address our priorities with respect to these expected projects, regardless of the status of JWST.		The Decadal Survey is vital for the astrophysics community to provide focal points for seeking government and external funding for our most important projects.
307	Yes	goals can b prioritized @ any time		to me, delay means loss
308	Probably not	Operational status, first light could be late 2020 assuming no problems, priorities on the table are useful if disaster happens. New discoveries from first year of JWST will shape other priorities if full success happens early.		Current schedule allows priorities to be ready for large scale efforts in restoring science and serious advances after 8 yr of dithering nothing by Obama regime. Delaying two years creates confusion and wastes of resources.
309	Yes	The prioritization should be based on science and cost, and recommendations could be contingent on the status of JWST and WFIRST as they become known.		It seems unwise to hold the entire process hostage because of the JWST delay, as the decadal survey has a broad scope (including ground-based OIR, radio, DOE, etc). Regular reviews are needed to keep our priorities updated given advancing science and technology developments and competition from abroad.
310	Yes	The survey is about scientific priorities. The funding realities will come later.		A delay risks extending the last decade's priorities into the next. Very much favor not delaying. No one will think less big if JWST is delayed. If less funding is available due to a delay, that will be reflected in later budgets. It shouldn't be reflected in science priorities.

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311	Probably not	The budget overrun of JWST will affect the funding available and may impact WFIRST, which in turn would affect technology development/demonstration critical for several possible future mission concepts.		
312	Yes	We have been living with JWST in the "background" for some time now. There's no reason to allow it to further derail the advancements in other areas with science and technology priorities. A delay will only cast doubt on NASA as a whole. This could potentially cause the public and government officials to question the efficiency of the institution and its funding levels. Now is not the time to demonstrate weakened resolve, which is how a delay may appear.		see last question
313	Probably			
314	Probably not	Two major factors: (1) The advance of observational astrophysical capabilities in several key areas (e.g., star/planet formation, young galaxies at high redshifts) depend on the presence of JWST. Thus the presence or absence of JWST produces a substantial change in which areas are best positioned to be pursued in the coming decade. (2) Funds. I would expect that whichever way JWST works out, it will impact the funds that NASA will have available for new projects. Thus the Decadal Survey needs to know JWST's operational status before proceeding.		I don't see many advantages to proceeding now. New science questions evolve with time, but also depend on technical capabilities. While I share the concerns about what we might lose by delaying, a DS that is disconnected by circumstances from scientific capabilities and the funding situation will not have the desired credibility. This is especially an issue with the wider community. Many of our productive young scientists now are in smaller institutions where they generally depend more on public access and modest scale funding opportunities than the majority of institutions represented in a typical DS core group.
315	Yes	I expect that JWST will work well and produce revolutionary science. However, the outcomes will not be fully appreciated – enough to assess the state of the field as a whole – for several years, which would be too long of a delay. Furthermore, if something did go wrong with JWST's launch or operations, the Survey could be delayed for a long time, creating a large effect on astronomy as a whole.		(1) It is essential for the community to maintain evaluation of its status and form plans on a regular interval. It is a problem that NASA's cadence for astrophysics missions is slow and a delay would exacerbate it. (2) I have heard that a delay would advance the Planetary Decadal Survey. I believe that is unrealistic: people who participate in the surveys – either as committee members or contributors – plan their career activities around these events and the disruptions would be large. Thank you for consideration my opinions.
316	Probably not	JWST is expected to have a big impact on astrophysics - any uncertainty on his status can not but reflect on future plans		The danger for keeping the current schedule is the loss of momentum for 2010 DS priorities (considering that JWST is a 2001 DS priority)
317	Yes			Decadal Survey is to be done every decade. JWST is delayed, but when will it really launch? The postponements could be endless. We astronomers should be seen as a profession that can stick to a plan.
318	Yes	Mission launches rarely coincide with decadal boundaries. If you can't have a decadal survey in the middle of missions, then there is no point at all in having a "decadal". Just have an evaluation (can't be called a decadal anymore) when the single flagship mission that NASA might be able to fly per 12 years is launched. Because that's what there's going to be. One astrophysics flagship every 12 years. In addition, the NAS should be given the opportunity to reevaluate prior choices before they get too far along. For example, if one sees, for instance, that a previous decadal priority might follow the lines of JWST and run over budget and past schedule, based on lessons learned from JWST, then one might expect that the Decadal survey might revisit those earlier priorities before they eat the astrophysics budget for a whole decade. Delaying the DS is a Really Bad Precedent.		I see the danger in delaying the DS, not in doing it on time. All the examples given could happen whether or not there is a decadal. If the DS can be delayed because of one mission, one deduces that the community, and NASA, are not serious about the decadal (one might have thought this when the "mid-decadals" started happening). If I were a young researcher, I'd look at the failure to evaluate priorities, and maybe decide to do something else. If you aren't working on WFIRST or JWST, nothing is going to launch in your lifetime.
319	Yes	There is sufficient information from current space missions and ground based observatories to proceed with the survey as scheduled.		I believe that the findings of current missions and ground based observatories plus prospective measurements with JWST before the completion of the survey provide sufficient information to proceed with the decadal survey on its current schedule; that is, no delay. We can envision a much higher throughput successor to the high angular resolution Chandra X-Ray Observation, named Lynx, as well as missions that observe gravitational waves and their high energy electromagnetic counterparts. There is no need to wait for results from JWST before undertaking the survey. If results from JWST were so profound that they motivate an entire reevaluation of mission priorities we will be able to deal with that.
320	No			
321	Yes	The priority can be made assuming successful deployment of JWST; a separate, brief consideration can still be made in case JWST is delayed.		Delaying the decadal survey will diminish US leadership, delay technology development, do harm to careers, and interrupt the pipeline of scientists.
322	Yes	The science drivers are known, they are currently being defined (with broad community input) by the large mission and probe studies. The JWST status is a technical issue, not a science one. We should be bold enough to be able to write these goals down so the next large mission can begin science and technical preparation, even if it is on a small, focused level. By investing modest resources into the next major priority will mitigate many of the cost and schedule problems that have plagued JWST.		I fear we will lose the 2020s to vague preparation and waiting around for JWST and WFIRST if we do not make a decisive recommendation on time. A decadal survey that does not finish meeting until 2022-2023 is 30% of the way to the next decadal, and the community needs actionable priorities before this. If WFIRST was sacrificed to make the next large mission priority feasible for a true start in the 2020s, this would be a valuable piece of information for the decadal survey to weigh in on.

	A	B	C	D
323	Probably	I think there are ways right now to assess the path forward under scenarios in which JWST is fully successful, or not. Even if the decadal was delayed for 2 years, it is unclear what additional information the community would have that would substantially change the outcome of the report. Any JWST data that will be available 2 years from now will still be preliminary, so it will still be too early for the JWST performance and science results to inform the decadal review process in a meaningful way.		I realize these survey questions are focused on space-based (i.e. NASA) priorities, and I can see some justification for delaying the decadal process if you only consider the ramifications for space-based astrophysics. However, I do not think the reasons for delay related to space-based astrophysics should (or do) outweigh the other reasons for keeping the decadal on schedule. The exercise of assessing the state of the field every 10 years is important for many reasons (e.g. assessing ground-based priorities, surveying the state of the astro community, modeling and theory considerations, etc.). Large space missions no longer go ahead on decade-long timescales due to their complexity, but that alone shouldn't be a reason to delay the decadal – otherwise one could envision a system in which the time between successive decadal surveys would grow longer and longer, with the rest of the (non-space-based) astronomy priorities suffering as a result.
324	Yes			
325	Yes	There are proposed missions, such as Lynx (formerly known as X-ray surveyor), whose science goals are not entirely dependent upon results from JWST and that are much needed as there are no other planned or concept missions akin to i.e. Lynx for next generation compliments in the electromagnetic spectrum.		This delay will put the US many more years behind other space agencies around the world in science goals and engineering feats.
326	Probably	JWST is unlikely to revolutionize my field (exoplanet direct imaging), and so priorities in my field can already be assessed before JWST flies.		I am concerned that delaying the decadal survey will hinder missions that are planned for the mid 2020s. Some of these projects are already 'in limbo,' waiting for feedback from the DS. Delaying the DS presents a significant risk that these projects will be delayed, causing cost over-runs, or perhaps even cancelled. My feeling is that a subsequent (perhaps mid-decadal?) review could and should update a 2020 DS, based on results from the early cycles of JWST.
327	Yes	some experiments (e.g. LISA) will not depend on the outcome of JWST, except perhaps financially		see above
328	Probably not			The real problem is that we have a backlog of flagships (JWST and WFIRST), both of which are having difficulties; this imperils the whole flagship concept. However, we have to keep in mind that the hardest part of a mission is when you've spent the whole budget, need a little more, and there's no end in sight and no data yet to remind you it was all worth it. The biggest danger is that we could lose the flagship line if we don't select something to follow WFIRST in the next decadal – indeed, in my opinion the only reason to do WFIRST itself is to protect the flagship line. WFIRST is a Frankenstein mission with narrow science objectives and no industry and little astronomical community support, it's primarily seen as a jobs program for GSFC and JPL. The pragmatic solution is just to build it to cost and you get what you get and be done with it. At this point, I think delaying the decadal is an option that is DOA. I would, instead, advocate for a mid-decadal specifically to select the next flagship mission. Not a great solution, but there are no great solutions, only crappy ones that leave us treading water. But not paving the way for a flagship beyond WFIRST imperils our hegemony in UV/optical/NIR space astrophysics. We've already ceded leadership in every other area of astrophysics, and we need to hang on to something. And the recent European selections are all but begging us to assume leadership in UV/optical/NIR space astrophysics.
329	Probably	https://docs.google.com/document/d/1XIMk3O4SbNAP1B_dOnT1CgQ3Jiuf0ny4F8nMY5DrfX4/edit?usp=sharing		https://docs.google.com/document/d/1XIMk3O4SbNAP1B_dOnT1CgQ3Jiuf0ny4F8nMY5DrfX4/edit?usp=sharing
330	Yes	Here is a another vote against any delay in the NAS decade survey		

Appendix B:

Text of the COPAG Survey on Possible Delay of 2020 Decadal Study of Astrophysics

COPAG Survey on Possible Delay of 2020 Decadal Survey of Astrophysics

The recent announcement that JWST's launch will slip, to no-earlier-than May 2020 and the proposed termination of WFIRST pose some concerns for the next Decadal Survey of Astronomy and Astrophysics.

The head of NASA's Science Directorate, Thomas Zurbuchen, and the head of NASA's Astrophysics Division, Paul Hertz, are concerned that the next decadal survey committee may not be able to effectively prioritize missions in the next decade due to uncertainties in the status of JWST and WFIRST. They have suggested that one way to resolve that concern would be to delay the next Astrophysics Decadal Survey by about two years. However, they are seeking thoughtful input from the community on whether there is another option - can we have an effective decadal survey even in the context of the uncertainties in the status of JWST and WFIRST? What intentional steps can we take to ensure that the decadal committee can effectively assess the highest science priorities and recommend a balanced program of activities and missions for the coming decade?

The COPAG (Cosmic Origins Program Analysis Group) Executive Committee would like to understand the thoughts of the science community on the expected value or possible dangers of an on-schedule (i.e., start in 2019, finish December 2020) Decadal Survey, particularly for space-based astrophysics. Please share your thoughts with us.

Please respond by 13 May, 2018.

Your name and email (optional)

Your answer

Professional status (optional)

- Early career
- Mid-career
- Senior level
- Other



Preferred NASA astrophysics science theme(s) (optional)

- Cosmic Origins
- Physics of the Cosmos
- ExoPlanets

Do you think it will be possible to prioritize astrophysics space science goals for the next decade before JWST's operational status is known?

- Yes
- Probably
- Don't know
- Probably not
- No

Why or why not?

Your answer

What advantages or dangers do you see to doing the Decadal Survey on the current schedule, vs. delaying the DS by approximately 2 years? (Examples might include: loss of momentum for 2010 DS priorities, ID of new science questions or technology challenges that require immediate attention/prioritization to avoid losing US leadership, better insight into new capabilities such as launch vehicle capability, impact on already planned activities, ...)

Your answer



SUBMIT